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ORIGINAL ARTICLES.

THE PRINCIPLES OF IMMUNITY AND CURE IN THE INFECTIOUS DISEASES.

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(Continued from page 400.)

THERE are three reasons (only one of which has heretofore been recognized) why the germicidal properties of blood-serum are greater than those of blood-plasma. These are: 1. The lessened amount in the serum of those substances that favor bacterial growth. 2. The concentration in the soluble nucleins that occurs as a result of clotting of the blood. And 3. The passage of the substance of the multinuclear cells into solution.

When I speak of the solution of the substance of the multinuclear cells I do not wish to commit myself to the theory that the entire cell breaks down. I refer simply to the fact—and I believe it to be a fact, as I have found nuclein in solution in blood-serum—that the nuclein passes from the cell into solution. Whether this process is due to the breaking down of the cell or to an active secretion on the part of the cell, I am as yet unable to determine.

As is claimed by Metschnikoff, there are obvious advantages to the animal in having the germicidal constituents of its blood (partly) in the form of ameboid bodies. Concentration at a desired place can be more easily accomplished, and the walls of the arteries and other tissues may be traversed without injury. But these cells do not eat and digest the invading bacteria. There are reasons for believing that they have no power to retard the progress of the invading bacteria until the constituents of the cells pass into solution. Germs often grow and multiply in these cells.

The fact mentioned by Metschnikoff as a proof of his theory that in the apyretic stage of relapsing fever all of the spirilla are found in the splenic cells, while in the pyretic stage they are free in the blood, is capable of a wholly different explanation from that which he attributes to it. It seems to me more likely that the spirilla seek the interior of these cells as a place of rest, where they reproduce themselves, and from which they go forth with renewed energy. In the monkey the splenic cells fail to afford this protection, and the pyretic stage does not return. Metschnikoff himself states that the malarial hematozoön, during the ameboid stage of its existence, finds its way into the corpuscle, and from the substance of this the parasite takes its nourishment.

The action of the soluble or alkaline nucleins on germs may be either inhibiting or directly toxic, or both. It is not necessary that the invading germ be killed in order to prevent its producing disease. If its growth and multiplication be inhibited the disease will not follow. Metschnikoff has shown that some of the germs taken even after many days from the local abscess formed in an immune animal inoculated with a pathogenic germ, and injected into a susceptible animal, may cause the disease. These nucleins are not only germicidal; they are also toxicidal. They render inert the bacterial nuclein, whether this be present in living or dead cells, whether it be in suspension or in solution.

In order to state my views upon immunity in a condensed form, and to bring this address within the limits of the time allowed, I will summarize as follows:

There must be three factors in the production of immunity in an animal naturally susceptible. First there must be an inciting or immunizing substance introduced into the body. We are acquainted in a rough manner with the nature of some of these substances, such as the venom of the snake, the vegetable proteids, abrin, ricin and robin, and certain bacterial proteids. I call these proteids, and I wish it to be understood that I do so tentatively, and that I recognize the fact that the exact chemical nature of none of them is known; but for the present we may call them proteids. For reasons already given, I believe that these proteids, which induce immunity, belong to the class of nucleins. I am aware of the fact that physiologic chemists do not always classify the nucleins among the proteids, but among the albuminoid bodies. These substances, be they nucleins, proteids, or albuminoids, have the property, when introduced into the bodies of certain animals, in certain amounts and under certain conditions, of so stimulating the activity of certain organs in the animal that these organs produce and supply to the blood an antidote to the substance introduced.

Secondly, the organs whose activity is stimulated by these immunizing agents are those, such as the spleen, thyroid gland, and bone-marrow, which manufacture nucleins.

Thirdly, the antidotal substance is a nuclein. The kind and amount of nuclein formed will depend upon the nature of the inciting agent and the condition of the organ or organs acted upon.

I use the word "nuclein" in a broad sense, including the true nucleins, nucleinic acids, and nucleo-albumins. By the term "nuclein" I mean that part of the cell which under normal conditions is endowed with the capability of growth and reproduction, which assimilates other proteids and endows these assimilated substances with its own properties. It is that part of the cell which gives it its individuality. Whether these nucleins, while in solution and devoid of morphologic unity, are still capable of assimilating allied bodies or not, cannot at present be positively determined. There is no *a priori* reason, so far as I know, for denying this. These different nucleins, that of the bacterial cell, and that of the blood, may be isomeric bodies, one of which may be designated as the α-compound, and the other as the β-compound, and one of these by simply causing a rearrangement in the atoms within the molecule of the other may assimilate the second. There are some good reasons for believing that certain proteid bodies contain a cyanogen-group, and Kossel has shown that nuclein contains a basis-substance, adenin, which has the formula $C_5H_5N_5$ and is a polymer of hydrocyanic acid, and Gautier has synthesized another constituent of nuclein, xanthin, by simply heating hydrocyanic acid in a sealed tube in contact with water and a little acetic acid. Indeed, the readiness with which cyanogen-bodies polymerize is well known to chemists. Thus, liquid chlorid of cyanogen C N Cl converts itself spontaneously, under certain conditions, into solid chlorid of cyanogen, $C_3N_3Cl_3$; cyanic acid into cyilid; methyl cyanic ether, C O N (CH_3) into methyl cyamin ether, $C_3O_3N_3(CH_3)_3$, and cyanogen into paracyanogen.

I do not mean to say that the process by which a nuclein assimilates another proteid is as simple as would be indicated by these examples, but they may suggest something of the nature of the chemistry of assimilation. We may suppose that when an alpha-nuclein and a beta-nuclein are brought together, one will assimilate the other; and which one will absorb the other will depend upon the relative strength or vitality possessed by them, and in measuring this strength several things must be taken into consideration. A vaccine has less of this power of assimilation than a germ of full virulence, and a nuclein in a sterilized culture less than either of the others.

An organ that has once been stimulated by a given excitant responds more quickly the second time to the same excitant, provided that the interval of time is not too great. This explains the gradual loss of acquired immunity.

Again, an organ that is stimulated by one excitant may not be responsive to another, and this accounts for the fact that an animal rendered immune to one disease remains susceptible to another.

The poison that is introduced is destroyed by the activity of certain cells. In order, however, to accomplish this destruction, it is probably not necessary that the poison should come in contact with the cells. For instance, the activity of the spleen has been found necessary, as the experiments of Tizzoni and Cattani have shown, to the production of immunity to tetanus. We can suppose that the process of immunizing an animal proceeds in something like the following manner: The modified virus of tetanus is introduced into some distant part, in some unknown way; the spleen is stimulated to action, and secretes a nuclein which is carried partly in solution, partly in the form of multinuclear cells, to the invaded part of the body, and the tetanus-poison is converted into the nuclein coming in contact with it, or is otherwise rendered inert. Later a larger quantity of the tetanus-poison is introduced, and now the spleen acts more promptly and energetically than before. This promptness and energy of action are increased by exercise, and finally an amount of tetanus-culture, of full virulence sufficient to kill an animal whose spleen has not been subjected to this training, may be introduced without ill effect. On this theory the production of immunity consists in a special education of certain cells, and artificial immunity becomes essentially cellular.

The difference between immunity and tolerance I conceive to be this: In the former the cells of certain organs become aggressive; a special function is developed; the poison introduced is destroyed. In tolerance, there is no aggressive action on the part of any organ; there is no development of special functions; the poison introduced is not destroyed; it only fails to kill.

Now, what can be said about the relation between the principles of immunity and those of cure? Are they the same? I think that there are essential differences. In the first place, the substances with which immunity is induced are not applicable in the production of a cure. They are already in the body, and have failed to stimulate the nuclein-forming cells in such a manner as to cause their own destruction. To introduce more of the bacterial poison after the invading virus has established itself in the system will only strengthen the invader. No disease, after being well developed, has been cured by the administration of the bacterial poison. I believe that vaccination, first performed after exposure to smallpox, may altogether prevent the development of the disease, or may modify it, but this is not cure. Of what service would vaccination

be when the body is already covered with smallpox-pustules?

Some experimenters have found that if a certain substance be introduced into the animal simultaneously with, or a few minutes, or at most a few hours, after the introduction of the virulent germ, the disease either does not appear, or it appears later than it does in control-animals, and they have claimed that these are instances of cure. Thus, Behring speaks of *curing* diphtheria in guinea-pigs with iodin trichlorid. He injected this "curative agent" immediately after inoculation, and at the place of inoculation, and found that, of the animals thus treated, some, although they sickened, did not die. He may have induced immunity in this way by the action of the chemical agent on the bacterial proteid, though this has been experimentally denied by Tizzoni and Cattani, but certainly such results do not deserve to be designated as cures.

Koch attempted to cure tuberculosis by introducing the proteid of the specific bacillus into patients. Theoretically this was a mistake, because this proteid in living, virulent form, is already present in the patient, and, indeed, is the cause of the disease. Immunity to tuberculosis has been tested by Nature in each tuberculous person, and has been found wanting. Some step in the process has failed. Either the poison was first introduced into the body in a form too virulent to be resisted, or the cells of the body have been unable to act with normal energy.

If I am right concerning this difference between the agents of immunity and cure, to what source shall we look for curative substances in the infectious diseases? Either we must introduce into the body some germicide formed by other cells, or we must employ other agencies for the purpose of stimulating the nuclein-forming cells. Blood-serum therapy offers the first of these alternatives, and now that we know that the germicidal constituent of the blood is a nuclein, blood-serum therapy will give place to nuclein-therapy, and with the latter there is more hope of accomplishing good results, because it reduces the size of the dose.

I say that the germicides from which we now hope for good results in treatment must be of cellular origin. The experience of the medical profession for the past twenty years has demonstrated that there is no known chemical substance which can be introduced into the animal body in sufficient quantity to effect a cure by its germicidal action without harm. Time after time the discovery of a non-poisonous, powerful germicide has been announced, but experience has invariably shown either that the substance is poisonous or that it is not germicidal. This universal experience of the profession has been scientifically demonstrated to be a fact by the researches of Behring, who after a large number of experiments came to the conclusion that, computed on the body-weight of the animal, antiseptic substances reach a fatal dose when injected in absorbable form under the skin in one-sixth the amount which is necessary to prevent the development of the anthrax-bacillus in the blood-serum.

Now that we have learned that the animal body itself generates a germicide more powerful in its action than corrosive sublimate, and since we know how to increase the amount of this substance in the blood, and can isolate it, and inject it into other animals, renewed hope comes to us.

I have stated that one advantage which nuclein-therapy will have over blood-serum therapy is to be found in the fact that it may enable us to employ the active substance in larger doses. We have seen all along that quantity as well as quality must be taken into consideration in the production of both immunity and cure, and that the effects are in proportion to the quantity. Even the ultra-bacteriologist does not now claim that a single bacillus of the most virulent kind introduced into the most susceptible animal will cause disease. The animal body is no longer to be regarded as a mere culture-flask, in which germs grow without meeting with the slightest resistance; and after the germs of tuberculosis have gained the ascendancy, and are multiplying by the millions daily, possibly hourly, we need not hope to find a tuberculin which in doses of the fraction of a milligram will, within a few days, or within any period of time, destroy this great mass of living, growing poison. The miraculous effects of small doses need not be looked for. These will have no place in the therapy of the infectious diseases.

If it be possible to kill the germs or destroy the bacterial poison after the development of an infectious disease by the introduction of a germicide or a toxicide formed by other cells than those of the infected person, then we may expect that cures for diseases of this kind will be found in the near future. Experimentation offers the only means of ascertaining whether or not this be possible. The recently reported cases of tetanus successfully treated with the antitoxin of Tizzoni and Cattani, obtained from the blood of animals which have been rendered immune to this disease, are in accord with this principle.

If nuclein-therapy fails us, we must strive to find agents that will stimulate the nuclein-forming glands. This probably is the chief factor in the climatic treatment of tuberculosis, but so far as our knowledge of medicinal substances that will accomplish this result goes, we are practically and wholly ignorant. The field of special cellular therapeutics lies before us a *terra incognita*. Im-

portant discoveries in this direction are not likely to be made soon.

I have used the word "cure," limiting its meaning to the destruction of the germs or other poison. If we could destroy all of the bacilli in the body of a tuberculous patient, would a cure be effected? If we ever reach this desideratum, Nature will probably do the rest.

I have thus imperfectly placed before you certain ideas that have for some years been slowly formulating in my mind. They have enabled me to demonstrate, first, that the nucleins are powerful germicides, and, secondly, that the germicidal constituent of blood-serum is a nuclein. Whether or not they will lead to further elucidation of the perplexing problems of immunity and cure I cannot predict.

EXOPHORIA CURABLE WITHOUT OPERATION.[1]

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AT the meeting of the Ophthalmic Section of the Pan-American Medical Congress, recently held in Washington, I said in the course of the discussion on Muscular Errors that operative measures for insufficiency seemed to me a very unphysiologic procedure; a hasty renunciation of distinctively medical or natural attempts at cure; without analogy in the treatment of similar incoördinations elsewhere in the body; requiring an impossible delicacy of execution; without definite prognosis; and very often utterly unsatisfactory as to results. Without offering anything to take the place of tenotomy, to which the whole ophthalmologic world seems committed, the statement may have appeared to some as absurd, and in the extemporaneous word then only permissible, I fear the plan of treatment I suggested was not clear to the members present. I have received so many inquiries as to the method that, although most of my cases are recent ones, I shall endeavor to make the matter plain by a few details and illustrative instances.

I now wish to go so far as to state that my experience shows that every case of exophoria (and possibly of divergent strabismus) is certainly and quickly curable by a rational and common method of ocular gymnastics—*of a certain kind, however, mark well!*—and absolutely without operation.

Let me first premise a few general statements that must have been apparent to candid investigators. The first is the absurdity of cutting a relatively strong, but in reality not over-strong muscle, in order to give its weak counteracting muscle more strength relatively, or what amounts to the same thing, more "purchase." This ridiculous procedure rests upon the more ridiculous assumption that the cause of the incoördination lies in the muscles alone, a fact that no one has sought to prove. And even if the assumption were admitted, it would not follow that tenotomy is the proper method of cure. A hundred facts must crowd to the attention of every careful oculist, going to show that, at least primarily, the fault cannot lie wholly, or chiefly, and generally not at all, with the length, strength, or weakness of the muscles, *quâ* muscles, but that it is largely, if not entirely, dependent upon innervation and innervational coördination. I need not repeat many evident reasons for this fact, but here are one or two. By prisms, for example, bases out, test the full power of weak internal recti "muscles" to adduct at twenty feet. Make these "muscles" exert every grain of power they possess, lifting the heaviest weight possible. Let us say they can only overcome 12°. Now put on, instead of 12° prisms (total), twice 12°, and fix the patient's gaze on an object twelve inches distant, then slowly carry the object away, while the gaze is riveted on the object, and in one minute or less you have carried the object twenty feet away, and without resultant diplopia. If one can only by the extremest exertion lift with one hand a dead weight of two hundred pounds, can one in half a minute lift four hundred pounds, by lifting it slowly, or by any method comparable to the foregoing method of ocular lifting?

Another proof is this: Fix the patient's gaze on a near object, and by extreme convergence put the supposably "weak muscles" on an extreme strain; then, while keeping up extreme convergence, and without movement of the head, move the object on which attention is fixed to one side of the field of vision. One of the "weak muscles" now doubles or trebles its contracting power—an absurdity according to the theory of muscle-weakness.

Take only one other example, an actual case: A patient with 18° of esophoria has both interni cut completely off—no "guarded" or "graded" tenotomy, but the tendon and adjacent tenon-fibers cut clean off—reducing the former high esophoria to a temporary exophoria. In ten days he has his original 18° of esophoria back again!

By their failures to get permanency and desired accuracy of result, or by many other such considerations as I have suggested, careful and conscientious ophthalmic surgeons should have been convinced that invariable tenotomy as a cure for heterophoria is a dismal failure. A few striking successes (readily explainable as due to changed innervation following tenotomy, or to the curative action of operations

[1] Because it conduces to clearness; because the majority of troublesome heterophorias are exophorias; and finally, because, though applicable to all kinds of heterophoria, the method requires some modifications in its special application to other varieties, I limit this article to a consideration of exophoria alone.

per se, or to mental effect, pure and simple) do not neutralize the many wretched failures, and do not establish the procedure on any philosophic or physiologic basis. And this is because the central innervational nature of the entire abnormality is overlooked.

In any myopathy that leaves the flexors of a part, say the arm or leg, relatively weak, do we cut the extensors? With a functionally weakened, paretic, or undeveloped leg, do we maim the opposite leg to bring both into coördination? Do not physiology, medicine proper, common sense even, counsel the development of a weak antagonist rather than the crippling of the stronger fellow?

I frankly confess that these and many other like considerations tormented me in former days, when I was blundering with doubts and authorities, and perplexed and tormented by every case of heterophoria—an ass between two or three bundles of poor hay, doubtful whether to choose the suspicious bundle labelled, *Prisms to be used as permanent crutches*, or that thorny, much-praised bunch, *Graduated Tenotomies—Look out!* or, finally, that malicious bundle ticketed, *For some other Fellow!* Prisms, correcting the insufficiency, to be worn all the time, are a sorry delusion. Temporarily they give relief, but in a little while the patient returns with the weakness of the "muscle" doubled, and successive pilings of prismatic Pelions on nasal ossa grow ludicrously disgusting to patient and to doctor. To proceed with tenotomies, graduate, undergradute, or post-graduate, is a good way to make money—so long as the patient will return for new operations and will pay—but unless one is determined to become a professional tenotomist conscience wearies with the load it must stagger under, and soon cries, Enough!

One ray of hope came to me in these dark days; but everybody did protest so loudly that ocular gymnastics was an utterly useless proceeding that I came near flinging it aside. That it was so rational and physiologic, so in conformity with all the laws of hygiene and health, is probably why I did not throw the suggestion away untried. As every success depends upon the method of carrying out treatment, quite as much as upon the means of treatment, I thought in this fact might be found the cause of failure in ocular gymnastics. A very little practice and observation have shown this to be true. To those who with more vanity than virtue always cry, *This is all an old story; nothing new about that!* I have nothing to say. There is nothing new about life or jealousy, but there are many new methods of making them practical and effective.[1]

It should be understood that a large number of cases of heterophoria are due to anemia, kidney-disease or cord-disease, tuberculosis, syphilis, or some other systemic or cerebral disease. To potter about the local symptoms of a general disease and not strike at the fundamental cause is professional incompetence, and worse.

Then, again, another large number of these abnormalities are directly caused by ametropia, and directly cured by curing the ametropia. The tenotomomaniac is wofully likely to ignore this highly important truth. Moreover, when an insufficiency is not progressive, and produces no troublesome reflexes, it is best left alone. But when one has a permanent heterophoria on his hands, not dependent upon ametropia or disease otherwheres, and a heterophoria that produces reflex symptoms—then he may and must go to work with the local condition.

In a word my plan consists especially in carrying the loaded or weighted stimulus of convergence slowly beyond near-range vision, or, in other words, carrying the weighted convergence-stimulus from the *punctum proximum* to the *punctum remotum*, and by repetitive gymnastics fixing it there. Half-jokingly I gave this equation to a student:

$$\text{Exophoria} + \text{C.-s.}^{\textit{weighted}} + (p.\,p. \times p.\,r.) = \text{Orthophoria.}$$

This, of course, requires an explanation.

In near-range binocular vision, there is an extra power of innervation of the muscles gained by the stimulus of the presence of an object at the *punctum proximum* that at least temporarily enables the visual axes to meet more accurately and certainly upon the object. In exophoria this extra innervation may now be loaded or weighted by prisms, bases out, and by slowly and steadily withdrawing the object, one, two, three—twenty feet (the patient's gaze kept intensely fixed upon the receding object), the innervation of the weaker function is stimulated and increased without diplopia or relaxation of the muscular grip. This is what I mean by carrying the convergence-stimulus into the distance, or *p. p.* multiplied by *p. r.* Let me illustrate: A patient is before us in whom we have determined a power of adduction (at twenty feet) of only 10° instead of the 30° or 35° he should have, an abduction of 6° or 8°, and a manifest (twenty feet) exophoria of 6° or 8.° Put in the

[1] I have heard of a method as having been used by somebody, consisting in giving the patient single prisms to hold by the hand, and thus train the muscles to overcome stronger prisms. This would appear to me dangerous and ineffective. It is impossible for the most careful patient to hold such lenses accurately in place. The prisms must be put in "fronts" or spectacles, "divided," and their strength, method and length of application, accurately watched by the surgeon. I have had no time to examine the literature, if any exists. Mr. Funnyman will do this, doubtless, for me, and kindly inform the world how old the method is he never heard of or used before; but Berry, de Schweinitz, Fuchs, and Norris and Oliver, the latest books out, allude to no method of gymnastics in insufficiency, and least of all, none by carrying the weighted convergence-stimulus into the distance.

trial or spectacle frame, 10° prisms, each eye, bases out, and let the patient firmly fix upon a small, bright object or 20/40 test-letter, held at a distance of twelve inches from his eyes. Slowly recede, the patient keeping his gaze riveted on the object. If diplopia arise, return quickly to the patient and repeat. In normally healthy, young, or young adult persons, one will, usually, at once be able to carry the object twenty feet away without producing diplopia. Two or three repetitions of this will enable the patient in a minute or two to look all about the room without double vision.

The reaction (or strength of prism overcome without diplopia) varies according to the amount of the exophoria, and according to the hundred varying conditions of nervous vitality, length of duration of reflexes, idiosyncrasies, etc. In nearly every patient in whose case I have employed the method I have been able to double the power of adduction in five minutes, and often to reduce the manifest insufficiency proportionately. If the reaction is low, the defect high, the vitality asthenic, I then order prisms in "fronts," and instruct the patient how to carry on the gymnastic exercises carefully and intelligently at home, and to return at least once a week. If the reaction and vitality are high and the heterophoria low, a few daily visits, with exercise under my direction, soon reëstablish "muscular" balance and the normal relation between adduction and abduction.

I use the simple rod-test or prism-test at twenty feet to determine the measure of manifest insufficiency. This is accurate enough for all practical purposes, especially as this is not the chief test. It is the adduction-test that is important, and the determination of the relation between adduction and abduction. All tests and records are made at twenty feet. "Convergence-stimulus adduction" means the adducting power at twenty feet, after exercise, by carrying the weighted or loaded converging power from the near to the distant point. To test adduction and abduction I have had a simple "battery" of prisms made, running from ½° to 30°, arranged in progressive succession, each ⅝ in. by 1¼ in., bases laterad, the whole about a foot long—a device that greatly facilitates speed. The measurements of adduction, of abduction, and of manifest heterophoria are the first to be made, and to be daily retested, until normality is established. A proportion between adduction and abduction, of about 4 or 5 to 1, and an esophoria at twenty feet of from ½° to 4° or even 5°, I call normality.

Deficient adducting power is the source of trouble and the thing to cure. It is a remarkable fact, and one that often occurs, that we may have normality or balance of the muscles at twenty feet, together with a highly subnormal adducting power. I think this is the reason of failure sometimes to cure a case of asthenopia. No manifest or even latent heterophoria existing, the adduction-test is not made. But it is precisely this test that tells of the hidden weakness and cause of all of the trouble.

The essence of the treatment is to arouse innervation by the weighted convergence-stimulus carried into the distance, and to make the effect permanent by short periods of gymnastics. The length of each exercise-period depends on the peculiarities of the case. From five to ten minutes, I think about long enough, repeated several times a day. If given at the physician's office, several five-minute periods may be advised, with intermissions, and the strengths of the prisms varied with the reactions of the innervation. The patient should read a little each time, and be taught to carry the gaze slowly from near to distant objects, without diplopia, etc. When carrying the object away from the prism-handicapped convergence the patient must be taught and constantly warned to look intensely, and with riveted attention at the object. If diplopia supervene, and the patient cannot overcome it by strong-willed fixation, we must come back within the range of binocular fixation and repeat the recession. If still the grip slips, we have at first weighted the weaker function too much and must temporarily reduce the load. But in all cases there is a steady rise of the innervational centers to meet the demand, and I have in healthy people never failed to double and sometimes to treble subnormal adduction within two, three, or at the most five minutes. At the next period of exercise this gain has only in part been lost, and in the beginning of the treatment the gain in adducting power at succeeding visits is "by leaps and bounds."

The question arises as to the attainment of permanent or continual normality, and the ability to discontinue the method. Every case varies. A few days of "treatment" suffice in low degrees of defect, and with good health and nervous vitality, to bring about normal coördination and adduction. In the more chronic cases, or in those of high degrees of heterophoria, or again in those with subnormal vitality, the return to normal balance is slower, some requiring a week or two, others a month or two. But in every case, even the worst, there has been relief or improvement, and progressive approach to balance.

A highly important point is intelligent, accurate judgment in loading the convergence-stimulus. Success will depend upon precision in the amount employed. But response to surprisingly heavy loads is remarkable, and increase of the load is allowable with astonishing rapidity, while one established grade of strength must be quickly followed by an increase

of the load. Of course, one can go *too* fast and *too* far, although I have never had a patient complain of pain or even discomfort from the severest and most long-continued gymnastics. All this again illustrates the nervous origin, rather than the mechanically muscular origin of these defects.

As to the durability and perpetuation of once-gained normality, I have been delighted to find how steadily it is maintained. Nature may often be unable to rid herself of a vicious habit or abnormalism of function, but after one little wise "lift" from the physician's hand, with a word of thanks and a good-bye, she is off, and thereafter she will keep things straight herself. There is, indeed, a difficulty in getting patients to keep up the treatment long enough to reinstate complete orthophoria. They get relief before this is entirely re-established, and see no reason for continuance. I have had no relapses so far, although I frankly confess that my oldest cases cured by this method have been only a few months out of my hands. Their reflex symptoms, however, disappeared before the return of muscular coördination, and subsequent retestings show no tendency to revert to the old disorder.

All of my cases had previously not been subjected to tenotomy. I hardly doubt that such a surgical history, if present, would make recovery by this or any other medical treatment more difficult, if not impossible. Just as surgical help is excellent beyond all things when it is needed, so when hygiene and rational therapeutics are alone required then surgical meddlesomeness and impertinence are most pernicious.

I append brief epitomizations of a few typical cases out of many that have been treated or that are still under observation:

CASE I.—Miss H., fifteen years of age, had much headache, despite correction of the ametropia and of the internal insufficiency by prisms, bases in. There was from 2° to 4° of exophoria, with adduction 22° and abduction 6°. Prisms, 10° each eye, were ordered and in ten days there was an esophoria of 2°, an adducting power of 32°, with ability to read as desired and no headache. The exercises were discontinued.

CASE II.—Mr. V., twenty-five years of age, had a vacillating exophoria, varying from 4° to 8°, and an adducting power of 18°. The adducting power increased steadily to 33° under gymnastic exercises with prisms, but the manifest exophoria showed no sign of lessening for two months. Finally, on September 23d, two and one-half months after beginning the treatment, muscular balance was reëstablished; adduction was 35°, and all headaches had entirely disappeared.

CASE III.—Mr. S., twenty years old, had received little benefit at my hands from glasses and prisms during a year previous to May 6, 1893, at which date my notes say there was exophoria 8° and adduction 12°. Gymnastics with six-degree prisms each eye, bases out, gave him on July 6th an adducting power of 23°. The exophoria still remained the same, but all pain had disappeared. I now ordered prisms, 10° each eye. On August 27th he overcame 35° prisms, total, bases out, but his insufficiency still remained about as high as before beginning the exercises. There was entire freedom of all previous ocular and cerebral distress. The gymnastic exercises were continued, and on September 30th I found muscular balance and a steady adducting power of from 30° to 35°. The patient was discharged happy.

CASE IV.—Miss S., twenty years of age, had on August 12, 1893, an exophoria of 12°, adduction 6°, abduction about 10°. Prisms, 8° each eye, were ordered, and in three weeks there was a desired but astonishing esophoria of 2°, and an adducting power of 28°, with much relief from headaches, etc. On September 1st the manifest esophoria was 3°, adduction 30°, with entire disappearance of headaches. Gymnastics were discontinued. At later visits the good conditions were still present.

CASE V.—Mrs. C., thirty-one years of age, had not had much relief from ocular distress and headaches with all the glasses and prisms I had ordered since 1891. On July 20, 1893, she had a manifest exophoria of 7°, an adduction-power of 8°, and abduction 10°. I ordered prisms, 8° each eye, bases out, in fronts, and taught her how to use them. In two weeks her exophoria was 2°, and adduction 30°. The headaches, etc., had disappeared.

CASE VI.—Mrs. T., aged fifty, had on July 25, 1893, an adduction of 6°, an abduction of only 3°, and at different examinations a vacillating high degree of exophoria of from 12° to 20°. Careful gymnastics in the office and at her home by the methods described had in three weeks halved the manifest exophoria, trebled the adducting power, and doubled the abducting power, with a complete cessation of the headaches.

CASE VII.—Miss S., twenty-five years of age, had been under treatment by several physicians prior to coming to me in 1891. For one year my attempts to relieve her reflex symptoms and absolute inability to use her eyes ten minutes in reading were as resultless as those of others. There was a fluctuating exophoria varying from three to eight degrees, and every device I had tried proved a sorry failure. On May 5, 1893, I gave her prisms, 6° each eye, bases out, in fronts, and with these she carried out ocular gymnastics several times a day as ordered. In one week her adducting power had increased from 10° to 25°, and the manifest exophoria had fallen from 7° to 3°. On July 10th all exophoria had disappeared, adduction was 30°, her reflex symptoms had disappeared, and her greatly increased power of reading was a delight to her. I have not heard from her since, and as she was a very faithful patient and visitor I think the cure permanent.

CASE VIII.—Mr. T., forty-seven years of age, had great redness and congestion of the eyes after he had read a little while. On September 2, 1893, he was found to have a manifest exophoria of 8°, ad-

duction 10°, and abduction 12°. On October 2d, after daily exercises with 10° prisms, bases out, his exophoria was 2°, adduction 28°, and no conjunctival congestion followed reading. He is still using the prisms, as I wish to produce an esophoria of about 2° and an adducting power of 35° before discontinuing the treatment, though he does not feel the need of improvement. Doubtless this has already been gained, but the patient lives at a great distance and can visit me but seldom.

CASE IX.—Mr. L., a young man of twenty-two years, came to me on July 14, 1893, with a simple hyperopia of 1.00 D., a manifest exophoria of 12°, and adducting power of 16°, abduction 5°. In one week's use of gymnastic prismatic exercises his adduction was 25°, his insufficiency 8° or 9°. On August 26th there was muscular balance and an adducting power of 35°. The gymnastic exercises were discontinued.

CASE X.—Mr. B., twenty-six years of age, had muscular balance, but an adduction-power of only 8°. In one week he could fuse prisms aggregating 27°, holding the image single at all distances and as long as desired, and his former trouble of headache, nausea, and dizziness had disappeared. I saw this patient three months later, and his adduction was 25° or more, and his convergence-stimulus adduction was 37°.

CASE XI.—Mrs. K., forty-eight years old, had been wearing, from another oculist, prisms, 4° each eye, bases in, for reading, divided for distance, but had constant diplopia. I found 4° of exophoria, an adducting power of but from 2° to 4°, and an abducting power of from 7° to 10°. After daily exercises with prisms for two weeks, 8° each eye, bases out, there was no manifest exophoria to be found, but I could develop 2° of latent weakness; the adducting power had increased to 12°, and, under convergence-stimulus carried to 20 feet, she fused 22°, bases out. One week later there was still the 2° of latent exophoria, but adduction had now become 20° and, with C.-s., 32°. The diplopia had ceased troubling her much. A week later no latent exophoria could be developed, the diplopia had disappeared, and the adduction-power was normal.

CASE XII.—Miss H., at her first visit had 8° of manifest exophoria, adduction 8°, abduction 8°, and a convergence-stimulus adduction developed in a few minutes to 25°. In two days the exophoria was 6°, the adduction 20°. In three days more the manifest exophoria had disappeared, but I could, with patience, bring out about 4° of latent. The adduction was about 30°, and the convergence-stimulus adduction over 35°. This patient was treated daily by myself, and illustrates the rapidity with which some cases react.

CASE XIII.—J. H. W., a lad of fifteen, had long had much trouble from blepharitis and conjunctivitis, despite the best correction of another oculist, and later of myself. I found a mostly-latent exophoria of 7°, adduction 11°, and abduction 7°. This was in my pre-gymnastic days, and I applied prisms, 2° each eye, bases in, together with the ametropic correction, and much attention was ordered as to school-work, exercise, the general health, etc.—all in vain. Gymnastic exercises, according to the foregoing method, were ordered, with weak prisms (6° each eye), and in three days his manifest exophoria was 2°, and adduction 22°. His constant prisms, bases in, were now replaced by his simple ametropic correction, and exercises continued at home. In two months there was orthophoria, adduction 27°, C.-s. adduction 35°. The result, I think, could have been reached in a week or two if I could have seen the patient often, exercising with heavier prisms, etc. His blepharitis and conjunctivitis had long ceased troubling him.

CASE XIV.—To illustrate the fact that even the highest degrees of insufficiency will react to this treatment, I will allude to a case that is yet incomplete, but which is steadily gaining ground every day. The patient is a little girl of seven, who had habitual alternate divergent strabismus, except when fixing an object attentively. From another oculist she had been wearing hyperopic lenses doubly over-correcting her ametropia, thus increasing the exophoric tendency. She kept the left eye closed a great deal. There was, on September 23, 1893, as near as I could estimate it, about 25° of exophoria, a power of adduction of only 2° or 3°, abduction 12°. I treated the child in my office for a while, and in three days she was able to fuse a total of 12° prisms, bases out, and with convergence-stimulus she could hold 20° at 20 feet. A week later she could at first fuse prisms bases out aggregating 27°, and after a few minutes' gymnastics she could hold 35°. The left eye was now kept open, the strabismus had disappeared, the exophoria was reduced to about 16°, and habitual binocular fixation reëstablished. The case, I judge, is proceeding to a perfect cure.

RUPTURE OF THE MALE URETHRA.

BY R. W. STEWART, M.D., M.R.C.S.,
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BY far the most frequent cause of rupture of the urethra is falling astride a resisting body, the weight of the blow being received on the perineum, and the urethra being forcibly crowded against the sharp edge of the triangular ligament, or against the symphysis or ascending ramus of the pubes.

Fracture of the pelvis, or even temporary dislocation of the pubic bones without fracture, is a frequent cause of rupture of the urethra, a fact that should always be borne in mind when one is confronted with these injuries.

Being thrown forward on the pommel of a saddle, or against the front wheel of a bicycle, or the old-fashioned treatment of breaking a chordee, may be included in the category of causes of rupture of the urethra, without by any means exhausting the list.

A discussion of the disputed theories of the exact mechanism of rupture of the urethra would be futile, as we have a condition to deal with, in determining

the means to be adopted for the relief of which no amount of theorizing from an etiologic standpoint would be of the slightest assistance.

The location of the rupture has a certain bearing on the prognosis and treatment, because, in a general way, the deeper the rupture is located in the urethra the greater is its gravity. Thus, ruptures of the pendulous portion usually owe their origin to but slight degrees of traumatism, such as the breaking of a chordee or the unskilful use of instruments. In these cases the rupture is rarely, if ever, complete; retention of urine is seldom a disturbing factor, catheterism is usually a very simple matter, and the liability to infiltration of the tissues by urinary extravasation is less serious than in deeper injuries. These cases form the mildest grades of ruptured urethra with which we have to deal, and they may be disposed of by briefly outlining their treatment. This consists in periodically withdrawing the urine by means of a soft catheter until such time as the rent in the urethra has become sealed. This may take place in a few hours, or at the longest in two or three days.

It is difficult to conceive of a rupture of the prostatic urethra other than that produced by the surgeon in attemping to remove a foreign body or new-growth from the bladder through a perineal incision, and as such cases plainly do not come within the scope of this article they shall receive no further consideration.

Rupture of the bulbous and membranous portions of the urethra may be classed together, as it is frequently difficult or impossible to determine the exact site of the rupture, because the symptoms of both are often identical and but seldom distinctive enough to be of much localizing value. However, if a tense, fluctuating tumor has formed in the perineum, and the scrotal tissues are intact, we are justified in holding that the extravasation is confined within the layers of the triangular ligament, and that the rupture is necessarily limited to the membranous portion of the urethra. Conversely, if the perineum is intact and the scrotal tissues are infiltrated, we may hold with equal propriety that the rupture is anterior to the membranous urethra. This, however, is a distinction without a difference, when we come to consider the treatment of these injuries, and it frequently happens that the traumatic cause of the injury has been so severe as to break down the dividing barriers of fascia, so that a rupture in either situation, if the extravasation be unrelieved, may be followed within a day or two by most extensive urinary infiltration. In severe cases the urine may infiltrate the perivesical, the perineal, and the scrotal tissues, and travelling still further it invades the loose tissues of the penis, pubes, and abdominal wall. In one case I have seen it invade the subcutaneous tissues of the thigh, from Poupart's ligament to the knee. The aspect of such a case is a most striking one: the bulging perineum, the enormously distended scrotum, the distorted phimotic penis, and the thickened belly-wall—all blackened by the infiltrating bloody urine, together with the agonizing distress of the patient suffering from the worst of all tortures, retention of urine, make an impression at once as lasting as it is unmistakable.

This is a picture of the worst cases with which the surgeon has to deal, but it should not be forgotten that it not infrequently happens that the rupture of the deep urethra is so slight, and the amount of urine extravasated is so small, that Nature is enabled by means of inflammatory exudates to limit the extravasation to narrow bounds, the extravasated urine, if not interfered with, either undergoing absorption, with a return of the parts to their normal condition, or if the urine undergoes decomposition, setting up a destructive inflammation with, at the worst, the formation of a perineal fistula.

Cases of extensive extravasation of urine, if left to the unaided efforts of Nature, are as surely fatal as would be the uninterfered-with course of a strangulated hernia. It is true that in extremely rare cases a strangulated hernia may by sloughing and abscess-formation find a safe exit for the intestinal contents; it is equally true that the vesical contents may find a similar exit by sloughing of the infiltrated tissues; but the chances in either case of the patient surviving until Nature has brought relief are so slight that no hope should be entertained; nor should a minute of precious time be wasted on the illusory efforts of Nature to effect a cure.

In the treatment of a rupture of the urethra with urinary infiltration we must promptly meet two urgent demands: First, to give free vent to the urine which has escaped from the bladder and permeated the loose connective tissues; and second, to relieve the distended bladder and afford a natural exit to the vesical contents.

In regard to the method of relieving the tissues of the extravasated urine there need be no controversy—early and free incision has been the recognized treatment in the past, as it will be in the future. In making these incisions it should be borne in mind that if ammoniacal decomposition of the urine has not set in, the urine has but slight irritating properties, and Nature may, as a rule, be safely left to take care of the tissues, provided free drainage has been afforded through perineal and scrotal incisions.

The best method of accomplishing the second indication, namely, to afford an exit to the vesical contents through the natural channel, is still in some respects a debatable question. The ideal method of accomplishing this would consist in passing a cath-

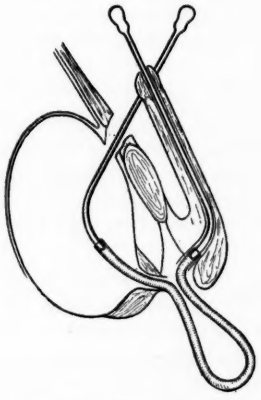
eter through the anterior urethra past the rupture and into the urethra beyond to the bladder, and retaining it there until the wound in the urethra has become sealed. A diligent, but not too protracted effort should be made to accomplish this end, and it will often meet with success, but in a large proportion of the serious injuries to the urethra, especially when the rupture is complete and the torn ends are separated by from half an inch to an inch and a half, it is simply impossible, even with the greatest skill and care, to introduce a catheter into the bladder. In these cases we are confronted with what has proved to be one of the most difficult problems in surgery, the ultimate solution of which, if treated according to the methods laid down in the standard text-books of surgery, is, to say the least, problematic.

The recognized treatment of the past has been either to resort to supra-pubic aspiration for the relief of the distended bladder and to trust in Providence for a cure, or else to boldly attack the situation by attempting to find the proximal end of the torn urethra through a perineal incision—a feat which may be easy on the cadaver, but is usually difficult and often impossible on the living patient, owing partly to the depth of the wound, but still more to the impossibility of recognizing the disorganized tissues in which the urethra lies. The history of such cases often is, that the operator blindly gropes his way in a deep wound, the bottom of which he cannot see, in search of a urethra he cannot feel, until, goaded to desperation by the futility of his efforts, he does by foul means what he cannot do by fair—he forcibly thrusts a catheter, regardless of the urethra, into the bladder with results that are, to say the least, disastrous. I have twice seen this done by competent surgeons, death resulting in one case and all but death in the other.

Difficult, dangerous, and uncertain as is the perineal method of attempting to restore the continuity of a ruptured urethra, it has the weighty endorsement of such text-books on surgery as Holmes, Bryant, Gross, and Moullin. Ashhurst's *System of Surgery* recommends opening the urethra through the perineal wound at a point beyond the rupture and performing retrograde catheterism—a feat which would be just as difficult and uncertain as finding the torn end of the urethra. Even the most modern text-book of surgery in our language, the *American Text-book of Surgery*, gives but a half-hearted endorsement of a method infinitely superior to the one we have been discussing. It says: "If persevering and careful search fails to reveal the proximal end of the torn urethra, supra-pubic cystotomy for the purpose of retrograde catheterism is justifiable, the distal end of the torn urethra being almost always easily discoverable."

The comparative advantages of the perineal and supra-pubic methods of treating rupture of the urethra are overwhelmingly in favor of the latter, a fact which will become obvious when we consider the technique of the latter operation.

The method I have adopted is as follows: The scrotum and perineum are first incised, to give vent to the extravasated urine; the incision in the perineum will enter a pocket containing bloody urine. The distended bladder is opened above the pubes by an incision just large enough to admit the index finger, which should at once feel for the vesical orifice of the urethra. A medium-sized steel sound is insinuated by the side of the finger and into the vesical orifice of the urethra; the finger should then be withdrawn from the bladder and the sound thrust along the deep urethra until its tip appears in the perineal incision previously made for the relief of the extravasated urine. Over the projecting tip of



the sound a rubber tube or, more conveniently, a soft catheter with its blind extremity snipped off, is forcibly thrust, and the sound, carrying the catheter with it, is withdrawn from the deep urethra into the bladder. The catheter is then detached from the tip of the sound and the latter is passed through the anterior urethra, until its tip likewise presents in the perineal incision; the end of the catheter which projects from the perineal wound is fitted over the tip of the sound which is withdrawn from the urethra, as is shown in the accompanying diagram, thus threading the catheter through the entire length of the urethra, where it is retained *in situ* by thrusting a safety-pin through it and one of the lips of the meatus.

If desired, the torn ends of the urethra may be approximated over the catheter by means of catgut sutures. The catheter should be retained in the urethra for from four to six days, after which the periodic introduction of full-sized steel sounds should be practised to prevent stricture-formation.

Of six cases of rupture of the urethra which I have treated I have succeeded in passing a catheter without resorting to operative interference in three. In two of these catheterism was only accomplished after it was thought impossible to do so, and the patients were placed in the lithotomy position preparatory to incising the scrotum and perineum.

In the three remaining cases I failed to pass a catheter and resorted to retrograde catheterism in the manner described. In one of these cases I attempted to find the torn urethra through a perineal incision, but failed. In one case there was slight leakage through the supra-pubic wound for eight days, in another for three days, and in one not at all. I may add that in all six of the cases recovery was satisfactory.

When we consider that in rupture of the urethra the bladder is distended from the associated retention of urine, facilitating its opening through a supra-pubic incision, and that the perineal incision for the relief of the extravasation is available for the threading of the catheter along the urethra, and also that no unforeseen circumstances can mar the success of the operation, the steps of which are as definite as those of an amputation, there can be but one conclusion: that the supra-pubic method should relegate the uncertain perineal method of treating this dangerous injury to the same obscurity that the supra-pubic method has relegated the perineal method of extracting stone from the bladder.

Westinghouse Building.

OXALIC ACID AS AN EMMENAGOGUE AND OXYTOCIC.

By HOMER C. BLOOM, M.D.,
of Philadelphia;
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It has been denied that there are such agents as emmenagogues, because "the discharge from the uterus is not one of the excretions through which medicinal agents pass out of the system."

This is but one of many theories which practice proves to be false. There is not an organ in the body that responds to medication more promptly than the uterus. Trousseau asserts "that all general excitants may be emmenagogues," as the uterine system does not escape the stimulation which these agents produce throughout the entire organism. I wish to call attention to one emmenagogue that I believe worthy of special confidence—oxalic acid.

How it acts, or why it acts thus, I am not prepared to say; theoretically I can give no explanation, and, indeed, no one seems to understand its peculiar action in this direction; but from practical observation it acts, in a large majority of cases, as the surest emmenagogue we can employ.

It is two years since I first observed a little article taken from some foreign medical journal relative to the possible value of oxalic acid in amenorrhea. I made a note of it, and in a short time found an opportunity to prescribe the drug.

Case I was in a girl, eighteen years of age. She was well developed, and for three years she had had all the premonitory symptoms of puberty—a monthly periodicity of associated symptoms characteristic of commencing menstruation; peculiar sensations, such as lassitude, pains in her back, gastric disturbances, irritability, flushes, with at times severe hypogastric pains. Frequently, at these periods, sympathetic breast-soreness occurred, in which the mammary glands were so irritable that the slightest pressure of the clothing gave pain.

I ordered the following prescription:

℞.—Acid. oxalic. . . . . gr. viij.
Aquæ, Syrup. limonis } . . . āā f℥ij. M.
Sig.—A teaspoonful four times a day.

After taking the medicine for three weeks the girl reported that during the previous week she had seen her menses for the first time. They had lasted one day and a half, were free from pain, the flow soiling three napkins.

She was asked to continue the prescription as before ordered. At the expiration of five weeks she reported that her sickness had come on at the expiration of twenty-eight days, lasting three days, and being perfectly normal.

The drug was discontinued and the girl was requested to report in three months, which she did, saying that her sickness had returned every twenty-eight days, and that she was in excellent health and had no symptoms.

I might add that in this case all the regular routine treatment for amenorrhea, or retarded or delayed puberty had been employed, such as hot douches, sitz-baths, and drugs many and various.

Case II was in an unmarried woman, twenty-five years of age, a native of Ireland. She had been perfectly regular in her menses prior to coming to this country, but never saw them after leaving Ireland. Her general appearance was fair; she had a good physique, was well nourished, and was a bright and intelligent girl. Her symptoms were flushes, with headaches, in which she complained of her head having "too much blood in it," with backache, etc.

I ordered the oxalic acid in quarter-grain doses, four times a day, requesting her to report from time to time, which she faithfully did, and after taking the drug for four weeks she reported that her monthly illness had come on for the first time, and she was entirely relieved of her symptoms, being "regular" thereafter.

This patient gave a history of having gone from one dispensary to another without any improvement.

Of twelve cases similar to the foregoing in which I have prescribed oxalic acid, and in which amenorrhea has existed for from six months to three years,

there were only two failures, and in both of these there was evidence of pulmonary trouble. I believe oxalic acid to be a drug that can be relied upon as an emmenagogue in all cases purely dependent upon some functional disturbance, of course bearing in mind that amenorrhea is not in itself a disease, but simply a result of some morbid condition affecting either the system at large or some part of the genital apparatus.

Probably there is no other disease in which the function of menstruation is interfered with to the same extent as in chlorosis, and it is in the treatment of this condition that I would ask for a more extended trial of this drug. I have had only three cases in which there was marked chlorosis. In two of these cases the drug acted quickly and promptly. In the other case, I found the anemia to be so profound that it was necessary to use iron in various forms before any improvement took place.

In several cases of scanty menstruation the drug has effected very good results. In others it has had no effect whatever. For example:

CASE III occurred in a married woman, thirty-three years old, sterile, with a history of the menses growing less in quantity and at greater intervals for the past three years, with lassitude, headaches, general irritability, nervousness, etc. Her menses last but a day and are very scanty and often six weeks apart. I ordered oxalic acid, in quarter-grain doses, four times a day, continued for six weeks, and normal and regular menstruation was reëstablished.

As to the action of oxalic acid, at or near the menopause, in those cases in which we find an absence of the menses for three, four, five, or six months, together with all of the phenomena attending menstruation, as well as the flushes and nervous condition incident to the climacteric period, I have in several cases seen decided benefit attend the increased menses, but in the large majority of cases the action has been negative.

As to the action of oxalic acid as an oxytocic I can positively say that it has decided ecbolic properties, having acted decidedly as an abortifacient in several cases.

CASE IV was in a woman, aged thirty, married six years, with three children, the last one born eighteen months ago. The menses appeared for the first time after the birth of her last child six months ago. They had been less in quantity than after previous pregnancies. She had flushes, nervous headaches, pains in her back, and constipation.

A saline was ordered and she was requested to report again. No examination was made. In the meantime her menses came on, but merely a show, with all of the phenomena and symptoms of the unaccomplished function.

Without any examination, I ordered oxalic acid in quarter-grain doses, four times a day. I had the mortification of being told by the same patient six days afterward, that she miscarried promptly after taking the medicine two days, and that she had had no untoward symptoms.

From the description of the products, as she gave it, I should think that she had been about two months pregnant.

A few days after this occurred, in speaking of it to my chief, Dr. Baldy, he told me of a similar case occurring in his own experience at the Pennsylvania Hospital dispensary, in which the patient returned and kindly requested the same prescription, "as it was one of the best she had ever taken to get rid of the unwelcome products of sexual intercourse."

The two cases just mentioned, with several others that I shall note, prove the ecbolic influence of oxalic acid in perfectly safe doses.

Probably the safest procedure for securing a miscarriage when medically necessary is operation, yet there are contingencies likely to arise which could best be subserved by the action of a reliable and safe oxytocic. This is my excuse for calling attention to the power of oxalic acid in perfectly safe doses as an abortifacient. The first case in which I prescribed it, as an ecbolic, is as follows:

CASE V.—Mrs. D., aged twenty-seven, had been married three years. She had had repeated miscarriages, at periods ranging from six weeks to three months. She had not seen her menses for six weeks, had obstinate nausea and vomiting, frequent desire to pass water, some mammary symptoms, constipation, etc. She was greatly reduced in strength and in the first stages of chronic pulmonary trouble. Her husband, also a patient of mine, is in the advanced stages of pulmonary tuberculosis. The woman had pulmonary hemorrhages, obstinate cough, a pronounced hectic condition, with other rapid progressive pulmonary symptoms. My efforts in the way of local applications to the cervix and uterus, as well as means directed to her digestive organs, and constitutional measures, availed nothing in the way of relieving her most obstinate nausea and vomiting.

The case was a desperate one. There were two results that seemed inevitable. First, the possibility of a miscarriage from an exhausted condition, and from the fact that she had had repeated miscarriages. Second, the death of the mother before the viability of the product of conception. To take the risks of the first was a course not only fraught with too much danger, but entailing the loss of valuable time, most likely the balance between life and death. The second was out of the question, for upon the life of the patient, however short, depended much.

With the certainty of either of the two inevitable results just mentioned, I hesitated no longer as to what was right; and there was every reason why this should be carried out with as little nervous shock as possible. With this before me and the fact of my previous unintentional experience with oxalic acid as an oxytocic, I made up my mind to try the drug. The patient was put upon quarter-grain doses

every four hours, in solution. After three days' use it had the desired effect, bringing on a miscarriage, followed by no untoward symptoms, but on the contrary complete relief from the nervous reflexes of pregnancy.

The remaining case, in which the ecbolic power of oxalic acid was proved, is as follows:

Case VI.—Mrs. H., aged forty-two, the mother of seven children, had the last pregnancy eighteen months ago. Menstruation was regularly reëstablished twelve months after the birth of the child, until eight weeks ago, when it ceased. The woman was sure from past experience that she was pregnant. Frequent desire to pass water, morning sickness, a bluish tint of the vagina and of adjacent soft parts existed. The cervix was soft, the uterus enlarged in its antero-posterior diameter, and the uterine arteries were easily felt.

The woman was nervous and anxious about her condition, for she was suffering from a large suppurating carcinoma of the left breast, involving the axillary and contiguous glands.

Here was a case that, in a few short months, would prove fatal. The history of her past pregnancies was such that additional suffering was more than she could bear.

There was no hope of her living until the viability of the product of conception, and there was every reason why she should be spared the additional suffering incident to her condition.

With this excuse, and the hearty coöperation of the patient and her husband, I therefore had no hesitancy in adopting such measures as would relieve the woman. She was put on quarter-grain doses of oxalic acid in solution every three hours. After three days she miscarried, without any ill-effects.

I have now used oxalic acid in some forty odd cases of amenorrhea, depending upon all the different causes, other than physiologic, which are likely to enter into the suppression of the menstrual function. Its reliability as an emmenagogue may be judged from the fact that it proved successful in three-fourths of the cases. As an oxytocic it proved its power in the four cases herein mentioned.

1427 Walnut Street.

ORIGINAL ADDRESS.

MEMORIAL OF PROFESSOR JEAN-MARIE CHARCOT.[1]

By PROFESSOR M. ALLEN STARR, M.D., Ph.D.,
PRESIDENT OF THE NEW YORK NEUROLOGICAL SOCIETY, ETC.

Gentlemen: It is my painful duty, at this the first meeting of the New York Neurological Society after the summer recess, to announce to you the death of our distinguished Honorary Member, Prof. Jean-Marie Charcot.

It is fitting that on this occasion we should turn from our ordinary proceedings and offer a tribute of respect to the memory of one whom we unite in considering the greatest leader in that department of medicine in which we have a common interest.

Charcot was born in Paris on the 29th of November, 1825. He was the son of a wagon-maker, a man of such limited means that he was not able to give all of his children an education. Jean-Marie, however, showed so much ability in school that he was selected from among his brothers as the most promising member of the family, and while one brother was enlisted as a soldier and another put to work in the father's shop, he was allowed to enter the Lycée St. Louis, at that time the best academy in Paris. After his preliminary education was completed there, the choice of the medical profession was made after some hesitation, for the inclination of the young man was for the life of an artist, and it was possibly the lack of means that finally determined him to take up medicine. After passing through the medical school and serving as an interne in La Salpêtrière—an almshouse for old women, then almost unknown, but later to become famous as the theater of his wonderful activity—he took his doctorate degree in 1853.

For the next three years he served as chief of the medical clinic in the medical school, supporting himself meantime by giving private tuition. He was then appointed physician to the Central Bureau of Paris, with a moderate salary, his duties being to examine applicants for the hospitals and assign them to the proper services. While filling these positions and slowly making his way, he was not idle, but was preparing for that competitive examination to which ambition for success leads every young physician in Paris.

It is to be remembered that hospital appointments in France are under Governmental control, and that as fast as vacancies upon the attending staff of hospitals occur they are filled by the appointment of men who have qualified themselves for these positions by passing a very rigid examination, by presenting theses, and on occasion defending in public debate the position taken in the thesis. Charcot succeeded in passing this examination in 1860, at his second trial, and it is said that he always supposed that he owed his success to his ability in defending the points made in his thesis upon chronic ulcerative pneumonia, which were ruthlessly questioned and criticised by his examiners. Later the discussion of theses was abandoned by the official examiners, much in opposition to Charcot's views, as he never forgot his own experience.

Having qualified for a position as attending physician to a hospital, Charcot was able to select in 1862 the service at his old hospital, La Salpêtrière. He found a large number of old people collected together in this poorhouse, and among them many with chronic incurable diseases. He had opportunities to watch the progress of disease, both acute and chronic, in old age, and, most important of all, he had unlimited pathologic material to supplement his clinical observations. It is to this latter fact that he owed the beginning of his reputation. For at that time pathology was just beginning to reveal the true reasons for diseases. It was an epoch of rapid progress in medicine. The nature of disease, the changes in tissues which caused the manifestation of symptoms, were just beginning to be recognized. Symptoms were no longer being regarded as diseases, but were being

[1] Read before the New York Neurological Society, October 3, 1893.

grouped together in classes and assigned to the actual pathologic process that lay at their base. If you will glance over any general treatise upon medicine written in 1862 and select the chapters that deal with nervous diseases, or rather with symptoms that we now know to be of nervous origin, you will be amazed at the degree of ignorance, in comparison with our present knowledge. It required a man of Charcot's ability in clinical observation, power of continued, earnest endeavor, talent for classification and broad generalization, diligence in pathologic study—a branch of medicine that requires unending patience, hard work, delicate manipulation, and constructive genius of the highest kind—to bring order out of the chaos of ignorance. By the careful study of his cases, by his power to seize upon salient features that present points for differentiation, by his persistent following of cases from the clinical ward to the autopsy-room, and by the exhaustive study of the material collected there, he began to reach conclusions, to arrive at facts which were veritable discoveries in the medical world. Study the files of the *Gazette hebdomadaire* from 1860 to 1870 and you will find a series of articles from his pen, each of which adds a distinct fact, observation, or conclusion to the sum of medical science.

These articles soon began to attract attention, and the young hospital physician found his rounds beginning to be attended by students who liked to watch his careful study of cases, or see him demonstrate the lesions that he found. In 1866 a small room in the hospital was set aside for the purpose of instruction and he began to hold clinics and to lecture. The lectures were soon crowded by graduates in medicine, and the next year he was invited to give an open course of lectures at the Ecole pratique of the medical school. Here he had an opportunity to cultivate the power of teaching and soon showed that wonderful facility of clear, direct statement and power of graphic illustration that later made his lectures the most attractive in the world. His first open course had for its subject cerebral hemorrhage and softening—an early indication both of his preference for neurology and of his firm belief in the need of pathologic study. In the following year he demonstrated for the first time the clinical and pathologic differences between paralysis agitans and multiple sclerosis, up to that time confounded under the title of "tremblement choreiform." There is little to-day for the writer on this subject to do but to present in inferior form the facts contained in those lectures. In his succeeding course he described the arthropathies appearing in the course of locomotor ataxia, which have ever since been known as Charcot's joint-disease. The next year the course was upon trophic disturbances following cerebral and spinal disease, and this course, given in 1870, necessitated the opening of a larger hall, as the number of students had already outgrown the small one. Thus, within four years, he had risen from the position of an unknown teacher to that of one of the best lecturers in the Paris school.

The year 1870, so full of sad events for France, brought a change in the service of La Salpêtrière. Under the exigencies of the war many buildings near Paris were destroyed and hospitals were in demand, and a new classification of their inmates necessitated the removal to La Salpêtrière of all female epileptics and hysterics that were not insane. This afforded Charcot new material for study, and from 1872 the study of hysteria in its multiform manifestations became one of his pursuits. This fact does not at once appear either in his writings or in his lectures. In fact, it was not until six years later that his work indicated any special interest in this new class of patients. But that the presence of this class at La Salpêtrière had its influence no one who has watched the recent investigations made within its walls will deny.

In 1872 Charcot was made Professor of Pathologic Anatomy in the Faculty of Medicine in Paris, the highest prize in the French medical world. For the ten following years he held this position, contributing in that time many facts upon diseases of the lungs, liver, and kidneys, and other organs, meanwhile, however, not neglecting his other lectures at the Salpêtrière upon diseases of the nervous system. It was these latter that were his favorite studies, and it was in this department that his work brought renown to himself and to France.

In 1882 this fact was officially recognized by the French Government, under the leadership of Gambetta —a personal friend of Charcot—by the foundation in the Medical School of a Professorship of Nervous Diseases, and Charcot's appointment to it, with the establishment of a public clinique at La Salpêtrière. From that time to the present year his annual courses of lectures have been attended by students of medicine from every land, and it may be truly said that there are few men of eminence at present living and working in neurology who have not drawn inspiration from his model clinics.

It was in the spring of 1883 that I had the good fortune to be numbered among his students, and it may be of interest if I relate some details of his work. On three mornings of the week, at nine o'clock promptly, he would come to the dispensary of the hospital and seat himself behind a little railing, which separated him on one side from the patients and on the other from the crowd of students, many of them foreigners, and all graduates in medicine. His assistants would then bring the patients, one by one, from the adjoining waiting-room. A concise history would be given of the case. Then Charcot would ask some searching questions, would elicit some unexpected symptom, would discover some physical appearance that others had not noticed; would examine the patient himself if there was any special point of interest, and then would quickly state his diagnosis, supporting the position taken, or discuss the probabilities or difficulties of diagnosis, often with interesting comments, and finishing occasionally with some therapeutic suggestion. Some patients were admitted directly to the hospital; others were referred to the assistants for treatment, but were not lost sight of, for it was not an infrequent occurrence for him to inquire, from time to time, about old patients, to have them brought in from the other rooms in order to note their progress or to compare them with some new patient, often with an appearance of personal interest that it was a pleasure to observe. After two hours of this work he would enter the hospital to make the rounds in the various wards. Here he would often stay till one o'clock, examining patients in bed or having them brought to one of the many rooms devoted to special investigations—to the eye-room, where careful tests of the visual fields for form and color were at that time engaging his attention;

to the photographers, to secure just the picture that he wanted of their deformity; or to the electric room, for tests or for therapeutic applications. He would often go to the bath-house, where he had had constructed all the various forms of douches and showers to which he attached much therapeutic value. He would also go to the laboratory and show us specimens, even slides under the microscope, of lesions, which had relation to the cases that we had seen in the wards. Lastly, we would gather in his own study—a room whose walls were covered with photographs illustrating disease in art, copies of celebrated paintings whose subjects were the healing of disease by miracle, a subject of which he always delighted to speak; for in the attempt of the painter to show the poor sufferer for whom the miracle was performed, he would detect a model suffering from hysterical contractures, or from hysterical convulsions, or some victim of a real paralysis. Many of these pictures have recently been published, with his comments, in the *Nouvelle Iconographie de la Salpêtrière*.

I remember that at that time there was a remarkable case of aphasia of the variety known as word-blindness in the hospital—a man who could talk and write; but could not read what he had just written—and Charcot would have him brought to his study day after day and put him through numberless tests, and then draw out some diagrams to illustrate aphasia, one of which he subsequently published in recording the case. This was the routine work of three days in the week, the best hours of the day being given up to his hospital service.

On Tuesdays the public clinic of the week was held in a large building just within the hospital gate. The room was arranged with a stage and footlights, and tiers of seats arose from the front level to quite a height at the rear of the room. As many as 600 students could be seated, and the place was always full. After the audience had gathered, dark shutters were closed at the windows, the footlights were turned up, and the clinic began. Charcot, attended by a number of his assistants, entered and seated himself on one side of the stage, at a little table, looking not at his audience, but across the stage. Then the patient or patients, for he usually showed a number at once, either to display variations of one disease, or to draw contrasts with other diseases, were placed before the footlights, and sometimes when a particular feature had to be demonstrated, a calcium light was turned on the patient, whose figure, the chief point of light in the darkness, could always be perfectly seen by all. In a clear but not loud voice, with a slowness of speech that led to a clear understanding—especially to the foreign element in his audience—Charcot would describe the case, call attention to special symptoms, show the peculiarities of spasm or deformity or tremor or gait, compare them with other similar forms for the purpose of differentiation, and sum up the diagnosis. Then, dismissing the patient, he would begin to describe the lesion, and at once on the screen at the opposite side of the stage the magic lantern would flash out the picture he was trying to draw, either in the form of a sketch made from Nature or an actual slide of a section of the spinal cord, or a part of the brain magnified by the microscope. Thus, symptomatology, diagnosis, and pathology were presented in orderly succession in a manner most clear and forcible, and with an effect that was most instructive and impressive. It has been said that the whole clinic was arranged for theatric effect. I believe that it was the only manner in which it was possible to demonstrate in a clear light to the large audience all the features, clinical and pathologic, of the subject. But grant that it was theatric; it left on the mind of the student a series of mental pictures of patients and of lesions which no amount of private study could possibly produce. It taught men so that they could not fail to remember, and what higher result can a teacher wish. And the students appreciated it. They gathered enthusiasm from Charcot's evident earnestness. No teacher has ever had such a constant company of devoted young men about him, eager to share in his studies, to be directed in their investigations, to be considered a part of his working force. He had the faculty of engaging their interest, of stimulating their investigations, of directing their work. He supplied them with material and showed them how to use it. As a result, there have appeared in France during the past fifteen years an enormous number of clinical and pathologic researches in the department of neurology, all of them inspired by Charcot, though carried out by some one of his pupils. He built up the school of the Salpêtrière. One can hardly name a young man of eminence in medicine in France to-day who has not at some time been glad to call himself a pupil of Charcot. And when, in addition to the long list of his own articles—his collected works will fill fifteen large volumes—the articles are collected on which his name appears as collaborator, there is presented a mass of medical literature far surpassing that of any other medical school in the world.

Bourneville has said that Charcot was not fond of writing or of publication, that it required two years of constant urging to induce him to publish the first two volumes of his lectures, and that he never would have given to the world many of his articles had some one of his assistants not attended to the details of publication. Fortunate then for the medical world, that he had these devoted assistants, by whom some of the drudgery of work could be carried, and through whom his marvellous knowledge could be spread abroad for the benefit of science.

It would take too much time to allude to his various books, lectures, and articles published between 1865 and 1892. I cannot, however, omit to mention his collection of cases of local lesions in the brain and spinal cord, which did much to establish on a firm basis the doctrine of the localization of cerebral and spinal functions, and which appeared in the *Revue de Médecine* in 1879, and his numerous valuable papers on the pathology of spinal lesions. It should also be known that, with Vulpian and Brown-Séquard, he founded the *Archives de Physiologie* in 1869; the *Progrès Médical* in 1873; the *Revue mensuelle de Médecine et Chirurgie* in 1877, now known as the *Revue de Médecine*, which still bears his name; the *Archives de Neurologie* in 1880; and the *Nouvelle Iconographie de la Salpêtrière* in 1888. To all these he continued to contribute from time to time until his death. It is said that no other medical work has ever approached the sale that his three volumes of lectures published from 1872 to 1880 have had. These lectures have been translated into German, English, Spanish, Italian, Hungarian, and Russian, and new editions are called for every year.

Public recognition of his abilities and eminence was not wanting. He had been president of all the chief medical societies in Paris, was a corresponding member of numerous societies abroad, of our own Society and of the Academy of Medicine in this city, and was made a member of the Institute of France in 1883 and Commander of the Legion of Honor in 1892. It may be justly said that France honored herself by conferring honors upon him, for there are few Frenchmen who, during the past quarter-century, have done as much to support the waning French authority in matters of science or to preserve the fading reputation of the French school of medicine. Not medicine alone, but French prestige as well, has reason to regret his untimely demise.

It has been made a matter of criticism that during the past few years Charcot's attention had been very largely directed to the study of hysteria and to the investigation of hypnotism and allied subjects. As we have already seen, it was force of circumstance that brought to his immediate notice this branch of nervous affections—a very large number of hysterical patients having been transferred to La Salpêtrière at the time of the war. When he turned from the study of organic diseases to that of functional affections he found the knowledge of hysteria in a chaotic state. Isolated observations had been recorded, but no definite classifications had been made. Theories abounded, but no real explanation or working-hypothesis on which to approach the subject was established. He began by grouping certain types together and clearly distinguishing them from others. He described in detail the more pronounced manifestations of hysterical convulsions, the epidemic forms of hysteria which had appeared all over Europe, in the past and even recently. He brought out clearly the sensory disturbances, the anesthesiæ, and visual defects of the hysterical sufferer. He called attention to the vasomotor and trophic symptoms of this affection. And thus, as one reads even the titles of his various articles, one begins to get an orderly view of the various types of the disease. With the facts of the affection before one, it is easy to follow Charcot into the more obscure ranges of speculation regarding its nature. Two conclusions largely established by his reasoning must now be admitted: *First*, that there is usually a basis for hysteria in a hereditarily degenerate nervous system; that its existence almost always implies some inherited weakness of brain-structure. *Secondly*, that, however various its manifestations, however distant from the central organs its point of appearance, hysteria is a disease of mind and brain alone, is central in its location. It is to prove this that all Charcot's researches into hypnotism tend. It was his great discovery that any hysterical symptom can be exactly reproduced in a healthy person by hypnotic suggestion—in other words, that the basis of hysterical manifestations is an abnormal mental state. The abnormal mental state being established, causes powerless to affect such results in health are potent to produce the peculiar manifestations known as hysterical. If these basal facts were the only outcome of Charcot's investigations upon this obscure subject, they would have been an ample return for the labor expended.

Charcot's investigations in hypnotism have attracted much attention both from the profession and from the laity. It is difficult for anyone to enter upon the study of this subject, or to pursue it with any degree of thoroughness, without subjecting oneself to adverse criticism and remark. One might have supposed that the reputation of Charcot would have protected him from such criticism, for never was one imbued more deeply with a desire to learn the truth, never was one more averse to that spirit of charlatanism which seeks notoriety or personal emolument. Charcot did not, however, escape the envious criticism of would-be rivals, or the ignorant comments so largely traceable to the public press. Even since his death one has read with regret the wholly erroneous estimates of his character which have been made in the daily papers by men of no knowledge, who allude to his recent work on hypnotism as if his reputation rested on that alone. It it difficult to estimate the true value of this department of his labor, but it cannot be denied that all through the years that he devoted to this study his mind has been imbued with the purest scientific motives; that facts and not theories were his aim. We may not accept his interpretation of the facts; we may even doubt some of his conclusions, but we must admit that the work has been done on a very different plane from that on which similar investigations had previously been conducted, and that, throughout, personal considerations were entirely in abeyance.

Another criticism that has been offered is that in his scientific zeal he neglected therapeutics, that his interest in patients ceased with the diagnosis. But those who make these statements must surely forget that our best methods of treating functional and hysterical nervous diseases, viz., the electric methods and the hydrotherapeutic methods, were successively developed and most successfully employed by Charcot. The use of baths and douches was introduced by him in La Salpêtrière many years ago; and to-day all over Europe, and I am glad to say at last in this country, the great remedial value of water-treatment is being acknowledged, largely through his influence. And even in the treatment of organic nervous diseases, where every scientific man feels that his object is at best merely to palliate suffering, as he cannot alter tissues, we owe some fairly useful methods to his experience and investigations. The use of the Paquelin cautery, of the ethyl chlorid spray, of the suspension apparatus, of vibratory therapeutics, may be cited.

It remains to allude to his personal characteristics. Charcot was a man of great dignity, of calm repose, of even temper, of slow thought and utterance, but of much reserve power. In appearance like Napoleon, and in manner reserved and observant, he was not the type of man to be popular. Yet his dignity was one that was felt to be appropriate to a man of great power and was never assumed. With patients suffering from trifling affections he showed no sympathy, possibly no interest. With patients whose diseases were grave, or obscure, or of rare type, he was kind, attentive, interested, and was ready to spend valuable time in most careful investigation. Of this I am assured by patients who had been under his care, and who would never have come away with such kindly feeling had they not experienced his thoughtful attention. His relation to his pupils was also one of mutual interest and affection. He was never familiar with them, yet he always respected and sought their opinions, was never autocratic in the

direction of their work, and was always the subject of their devoted admiration and respect. No man could have kept about him such a number of able medical workers, all loyal to him in the midst of their labors, without characteristics of many kinds of the noblest type.

His domestic life was a delightful one. He was married early in life to a lady of considerable wealth, who was enthusiastic in his work, and by whose aid many scientific undertakings otherwise impossible were carried out. He had two children, one a boy, whom it was his fond desire to see succeeding as a physician, and who gives promise of being worthy of the name. Their homes—he had a large country place at Neuilly as well as a fine mansion on the Boulevard St. Germain—were superb in every appointment and contained many treasures of art, for the artistic sense was one which he delighted to indulge. He was a designer, a painter on porcelain and in oils. The tiles in his study mantel and the ceiling of one of his salons were painted by his own hand. Many of the drawings displayed at his lectures and published in his books were drawn by himself. And he delighted in gathering about him curios of every kind. He cared less for music than for painting, and always preferred the classic and Italian schools to Wagner, showing himself thoroughly French in this respect.

Like every man of eminence he had his enemies, critics in science, rivals in medicine. Yet after all their criticism is exhausted, we must admit that Charcot remains the greatest French physician since Trousseau, the greatest ornament of the medical profession of the present age. We admire his genius; we esteem his scientific work; we respect him as the greatest of medical teachers; we honor him as a noble, unselfish, and truly great man.

CLINICAL MEMORANDA.

TRAUMATIC HEMORRHAGE IN THE RIGHT CEREBRAL HEMISPHERE; RECOVERY.

BY R. EMMETT GIFFEN, M.D.,
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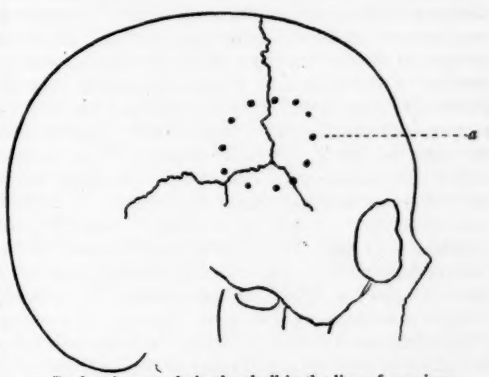
SURGEON-GENERAL NATIONAL GUARD OF NEBRASKA; SURGEON-IN-CHIEF ST. ELIZABETH HOSPITAL, LINCOLN, NEB.

D. Y., twenty-three years of age, a Russian, living in the squatter portion of the city, on December 8, 1892, while quarrelling with a companion, was struck on the head with a hatchet, the pole of the hatchet striking him on the right side above the ear and to the front, about the junction of the temporal, parietal, and frontal bones, and producing a lacerated wound.

He was seen by Dr. Britt soon after the accident, who found him in a dazed condition, with the scalp wounded as described, but with no depression of the skull. I saw him on the following morning, sixteen hours after the accident, and found him presenting all the evidences of compression of the brain; the right pupil was dilated and irresponsive to light; the left normal and responsive to light The skin was hot and covered with a profuse perspiration; the pulse slow, forty per minute and irregular; the respiration stertorous. During the night, the man's sister told me that he had had spasms of the left arm and leg, but that now these limbs were completely paralyzed, and the right was gradually becoming so. This, to my mind, was important, as it showed that compression was gradually taking place, involving one structure after another; and if something was not done immediately, death would result soon, or if not death, a permanent disorganization of brain-tissue with its results, which are worse than death.

My diagnosis was hemorrhagic clot pressing on the arm and leg center on the right side of the brain, possibly caused by fracture of the inner table of the skull, cutting the meningeal artery; and, as the man was sinking gradually, I advised opening the skull—not trephining, but raising a piece of the skull, after the manner of Wolfe, Horsley, and others. It took very little chloroform to affect the patient. I cut through the scalp down to the skull; then with a brad-awl, such as is used by carpenters, I cut holes in the skull over the line in which I intended using my chisel, in this manner:



a. Perforations made in the skull in the line of opening.

It is surprising how rapidly one can use the awl and make the openings, which weakens the line of incision so much that the chiselling is not difficult, as the bone readily gives way between the points of opening. Having raised the skull I found a large blood-clot, and the middle meningeal artery bleeding profusely. After turning out the clot, I tied the artery; then, upon examining the inner table of the skull, I found it fractured, and that a spicule of bone had been the cause of the trouble by cutting the artery. I removed some, and replaced other pieces, for the fracture was a stellate one, and was ready to replace the bone that had been raised up, but, in examining the broken, or under part of the hinge of bone, which is always broken by raising the piece of skull, and in passing my finger along the break, I found that a spicule of bone had been broken off by raising the piece, and had cut through and was pressing on the brain-tissue. I removed this, for there was ample room, and there would have been no excuse for leaving anything unfinished, and stopped the bleeding with styptic cotton. Then washing the wound thoroughly with hot water, I permanently closed it. Twenty-four hours afterward the man was sitting on the side of the bed with his trousers on, and he said that with the exception of the headache he felt well and did not see any use in staying in bed. I advised him to stay in bed for a few days, which he did. At no time after the operation did he have a temperature of over 100° F., and two weeks later he was on the

streets and able to attend the trial of the man who struck him. He is now working on the railroad, perfectly well. In this case, it was utterly impossible to use any antiseptic precautions, as the man lived in a hut surrounded by the filthiest of people—four of them living in the same room, all cooking, eating, and sleeping, in one apartment. The bed he was lying on was a feather-tick that had been brought from Russia with him; and for dirt, I never saw anything to equal it. They would not allow us to remove him to the hospital, so I was forced to do the best possible. I used plenty of hot water, but no other aseptic. Yet, as stated, this man's temperature never went over 100° F.

ACETANILID AS A DRESSING FOR WOUNDS.

BY FRANCIS W. HARRELL, M.D.,
OF GILMAN, WASH.

AS surgeon-in-charge of the Seattle Coal and Iron Company's mines at this place, and having to treat a great number of burns, scalds, contused and lacerated wounds into which coal-dust, ashes, iron-filings, grease, and dirt of every kind have been thoroughly ground, and finding none of our present dressings equal to preventing suppuration in these cases, I have naturally been searching for some means of making these wounds aseptic and keeping them so—some dressing that would prevent sepsis, and kill germs by contact. I think I have found what I wanted in acetanilid in the form of a powder. Reasoning in my own mind that many of the coal-tar derivatives have germicidal properties, and being struck by the non-hygroscopic properties of acetanilid, I began experimenting with it as a dressing for wounds.

The first case in which I tried it was an extensive laceration of the skin and muscles of the lower arm and forearm in a boy of nineteen. There were two wounds, one on the upper surface of the lower arm, an inch by two inches in extent, and one on the under surface of the forearm, an inch by two and one-half inches in extent. After carefully washing these surfaces with warm water, I applied pulverized acetanilid; the next morning I examined the wounds and found no pus, and then applied more acetanilid. The same result was observed each time I examined the wounds, and I also noted that they were growing perceptibly smaller. After a week the boy went off on a fishing excursion and I did not see him for a week. The dressings had not been changed during this time, but the wound on the forearm had healed, while the one on the arm was not larger than a dime. I applied a fresh dressing, and healing was complete in about a week. At no time was there any show of pus in these wounds. The young man called my attention to the fact that the dressing when first applied "was a little peppery," as he expressed it, but that in ten minutes all pain had disappeared.

The same effect has been noted in every case in which acetanilid has been used, clearly showing that it has local anesthetic powers. I have treated similar injuries with iodoform, boric acid, carbolic acid solutions, and with bichlorid solutions and gauze, but I have never succeeded in curing them without the formation of pus. Since then I have used acetanilid in the treatment of every case I have had in which there was extensive loss of tissue, and in every case it has acted well, the usual result being no pus and rapid recovery. I have not been able to use it in any major operations, as, fortunately, we have had no very severe accidents here for the last two months. My method of treatment has been cleanliness, with the application of finely powdered acetanilid to the wound. Apply plenty of it; in fact, for a crushed finger I make a bed of acetanilid.

Not being a practical chemist myself, and not having a hospital at hand where I can carry on extensive investigation, never having seen anything written on this subject, and being impressed with the cheapness of this substance, its lightness, its bland, non-irritating qualities when applied to abraded and wounded surfaces, I make this communication to call the attention of the profession to the value of acetanilid and to get someone who has more opportunities than I have to investigate it. I have found acetanilid a dry dressing that does not scald the skin, as does boric acid; does not have an offensive odor, as does iodoform; will never poison by absorption, as does sometimes carbolic acid or mercuric chlorid solution, or gauze.

MEDICAL PROGRESS.

Secondary Post-partum Hemorrhage.—At the last meeting of the Colorado State Medical Society, DR. JOHN T. DAVISON reported the case of a primipara, twenty-nine years old, who was delivered of twins at about the eighth month of pregnancy, being attended by a midwife. The children were syphilitic in appearance; one died twenty-eight hours, the other nineteen days after birth. The mother was up, engaged in her by no means light household duties, five days after the labor. She attended the funeral of the second child, riding some miles in an open wagon. Three weeks after delivery the woman was seized with profuse uterine hemorrhage. She was found lying upon the bed, quite pale and anxious. Her clothing and the bedding beneath her were saturated with blood. There was not even hot water in the house, and no attendant save a neighbor who had chanced in.

Bimanual examination showed the uterus well contracted, of the usual size for the period elapsing after delivery, and occupying the natural position. Upon introducing a duck-bill speculum, blood was seen issuing freely from the os. At this juncture the patient became quite blanched, sweat appearing on the forehead, and the pupils dilating. The emergency being pressing, the vagina was tamponed with the first accessible substance—a new, clean sheep's-wool sponge of about two ounces in weight. This was torn into shreds, soaked in a one to six solution of ferric chlorid, and introduced until the introitus was reached. In spite of this, considerable oozing from the orifice continued. This was subsequently thought to be due to the patient's previously anemic condition, or possibly to a hemophilic tendency. The oozing continued for about an hour, finally ceasing. Fluid extract of ergot was given in dram-doses every four hours for a few days, quinin, iron, and a liberal diet prescribed, caution enjoined as to the use of hot liquids, and small quantities of stimulants allowed. The tampon was removed thirty-six hours after its introduction; vaginal irrigations of mercuric chlorid, 1 : 3000, and carbolic acid, 1 : 50 (to destroy fetor), were employed. The tam-

poning with absorbent cotton and the irrigations were repeated daily for about one week; then discontinued. Nine days after the first profuse hemorrhage the woman concluded to sit up in a chair, when a second, but a lighter hemorrhage took place. Tamponing was again resorted to with success, as far as immediate control of the hemorrhage was concerned, but any attempt to discontinue the use of the tampons was followed, on slight exertion, by hemorrhage, so that their use was continued daily for some weeks. An inspection of the os, made previously to the introduction of the tampons, disclosed the existence of a bad laceration, but any attempt to ascertain the precise location of the torn artery resulted in the loss of a large amount of biood. In view of this, and of the patient's weakened condition, tamponing alone was relied upon to control the hemorrhages, and, as the sequel shows, with success.

Perhaps four weeks after the first hemorrhage, a semiglobular body was observed protruding at the os externum. This suggested partial inversion of the uterus, or a polypus. Its spontaneous disappearance after some weeks supported the former assumption. Under roborant treatment, with rest in bed and a liberal diet, the patient's general health and strength gradually improved, and she was soon allowed to get up, finally recovering fully and gaining many pounds in weight. About sixteen months after her first confinement the woman was delivered of a large male child, when the cervical scar could be easily felt by the examining finger, incidentally verifying the previous diagnosis of hemorrhage due to laceration of the cervical artery.

Sigmoiditis.—MAYOR (*La Semaine Médicale*, 1893, No. 46) has observed a condition of inflammation of the sigmoid flexure of the colon giving rise to symptoms analogous to those of typhlitis and perityphlitis. Constipation appears to play the principal rôle in the etiology of the affection, either mechanically or as a result of the absorption of toxic matters. The inflammatory process may be limited to the walls of the intestine proper, constituting sigmoiditis; or it may extend to adjacent structures, giving rise to a local peritonitis, and constituting perisigmoiditis. The principal and most characteristic symptom consists in the presence of a painful movable tumor in the left iliac fossa. In cases in which the inflammatory process is limited to the coats of the bowel the tumor is cylindric in shape and of considerable size, being lost below in the pelvic cavity and being continuous above with the descending colon. This swelling may be mistaken for the cord described as present in cases of enteroptosis; for an iliac phlegmon or adenitis; for simple gastro-colic dilatation; and for muco-membranous colitis. The sigmoid cord of enteroptosis is of smaller volume and more perfectly cylindric; it is also characterized by hardness and retraction. Inflammation of the iliac glands is attended with dulness upon percussion, while a certain degree of resonance usually persists in case of sigmoiditis. An iliac phlegmon is to be differentiated from an iliac peritonitis by the fact that the swelling of the latter is bosselated, not flat on percussion, and does not extend to the abdominal walls or to the pubic arch. In case of simultaneous dilatation of the stomach and sigmoid flexure, inflammatory symptoms are absent. Cases of muco-membranous colitis are usually associated with hysteria or neurasthenia and present pain and induration in the left iliac fossa; the mass present is band-like, and there is an absence of inflammatory symptoms, while the stools are characteristic. Patients affected with iliac enteritis sometimes, but not always, present fever. They are often emaciated, with coated tongue and anorexia. The stools are irregular and scybalous. The condition is curable, although sometimes obstinate. Should pus form it will require evacuation. Under all circumstances the patient should be put to bed and restricted to a liquid or semiliquid diet; occasional doses of castor-oil may be given; a copious enema is to be administered on alternate days; cataplasms may be applied over the painful region, and possibly mercurial ointment; tincture of iodin and a flannel bandage may also be applied.

"The Metapore (Foramen of Magendie) in Man and an Orang" is the title of a paper presented to the American Neurological Association, July, 1893, by Professor Burt G. Wilder, M.D., which was designed:

First. To call attention to the existence of at least two varieties of the metapore, viz., one in which the metaplexuses barely project at the margin, the other in which they extend for some distance upon the cerebellum.

Second. To record the discovery by the author of the metapore in an adult orang and in three Old World monkeys. In the former there are several orifices in the metatela.

Third. To admit the unsatisfactory state of our knowledge as to the structure and relations of the parts and cavities involved, and as to the functions of the arrangement.

Of the ten special questions asked in the paper, nine are discussed more or less fully in the *Reference Handbook of the Medical Sciences*, Supplement, 1893, pp. 615, 616, the tenth is as follows:

10. *Does the metapore represent an evagination of the metacelian endyma?* This is suggested as an alternative to the usual view of perforation, by Professor C. S. Minot (in a manuscript communication to Professor Wilder, dated July 17, 1893), who says: "The passage from my *Human Embryology*, quoted by Dr. Wilder in the *Reference Handbook of the Medical Sciences*, Supplement, p. 613, is not intended to throw any doubt on the existence of the foramen of Magendie or metapore. . . . It is an opening through the roof of the fourth ventricle. There are two possibilities: Either it is a break in the epithelium, or it is the orifice of an evagination." Dr. Minot does not state at what period the evagination hypothesis occurred to him; it is not even hinted at in his *Embryology*, in which, indeed, on page 676, the very existence of the metapore seems to be ignored. Hence, Dr. Wilder holds that any misapprehension on his part as to the actual nature of Professor Minot's views is due to the summary and inadequate treatment of the roof of the fourth ventricle, already commented upon in Dr. Wilder's review of the book in *The Nation*, May 11, 1893.

A Case of Leprosy resembling Morvan's Disease.—At a recent meeting of the Société Médicale des Hôpitaux, DEBOVE (*La Médecine Moderne*, 1893, No. 60, p. 758) presented a man, fifty-one years old, who fourteen years

previously had a whitlow upon the palmar aspect of the right thumb, incision of which was unattended with pain. For a number of years successive whitlows recurred in the same situation, which, though painful at first, subsequently became insensitive. There was, however, no necrosis of bone. In the course of time the right hand became contracted, claw-like, with a deviation to the ulnar aspect, and atrophy of the muscles of the thenar eminence. Common sensibility was preserved, but thermal and painful sensibility were lost on the right forearm up to the elbow. Morvan's disease was at first thought of, but subsequently a diagnosis of leprosy was made. It was learned that the man had lived in Mexico, and on investigation neuromata were found in various parts of the body. From the data obtained, the disease must have had a period of incubation of fifteen years.

Acute Parenchymatous Nephritis following Varicella.—CASSEL (*Deutsche medicin. Wochenschr.*, 1893, No. 32, p. 769) has reported the case of a girl, four years old, who had previously never suffered with any infectious disease, but who upon the sixth day of a febrile illness presented a typical varicellous eruption. The urine at this time contained no albumin, but nine days later, when she was brought on account of a swelling of the face and diminished renal secretion, the urine contained blood, albumin, and granular and epithelial tube-casts. In the following days the condition of affairs progressively improved and so continued to do until the child was lost from view.

THERAPEUTIC NOTES.

Theobromin for Cardiac Dropsy.—At a recent meeting of the Paris Academy of Medicine, GERMAIN SÉE (*Allg. med. Centralzeit.*; *Wiener med. Pr.*, 1893, No. 34, p. 441) reported excellent results from the employment of theobromin in the treatment of grave cases of cardiac dropsy. Theobromin, like caffein, belongs to the group of xanthins, which are closely related to uric acid. Theobromin is dimethyl-xanthin, while caffein is trimethyl-xanthin. Theobromin was administered in daily doses of from 45 to 75 grains for three days successively, with excellent results. The amount of urine was increased at once and the diuresis subsequently became still greater. The drug was employed in cases of cardiac dropsy, valvular or myocardial in origin, independently of whether ascites coëxisted or not. It proved inefficacious in cases of ascites dependent upon nephritis. Theobromin has the advantages over other diuretics, such as digitalin, caffein, calomel, milk, and milk-sugar, that its use is unattended with unpleasant result of any kind, that its action is more certain, and further, that the ordinary diet is not interfered with. The use of so-called theobromin sodio-salicylate is cautioned against, which is said to be only a mixture of theobromin and sodium salicylate.

A Remedy for Dysentery.—MIXON (*Texas Sanitarian*, vol. ii, No. 10, p. 414) recommends choparro amargoso, a small thorny bush that grows in Southwest Texas, in the treatment of dysentery. The bush belongs to the natural order Simarubaceæ, which also includes quassia and simaruba. All parts of the bush possess medicinal virtue, except old stock. The bark, leaves, thorns, flowers, and fruit, all have an intensely bitter and characteristic taste. The tendrils contain the active principle in greatest proportion and are generally selected for pharmaceutic purposes. The plant yields its virtues to boiling water, in the proportion of from two to four ounces to the gallon. The dose of the fluid extract is from ten minims to half an ounce as a tonic, from three drams to an ounce as a stimulant. Large doses cause flushing of the face and fulness of the head. The odor of the drug can be detected in the urine, although no appreciable effect upon the renal secretion has been observed. The drug has some antiperiodic properties, which might make it a desirable remedy in the treatment of malarial dysentery. The active principle upon which the properties of the drug are supposed to depend has not yet been isolated.

Aconitin Nitrate in the Treatment of Erysipelas.—At the recent Congress of the French Society for the Advancement of Science, TISON (*La Médecine Moderne*, 1893, No. 65, p. 822) reported the employment of aconitin nitrate in the treatment of some twenty cases of erysipelas, with the result in every instance of diminishing the pain and shortening the duration of the attack. Complications developed in but two cases, inflammation of the knee-joint developing in one, and an abscess of the temple in the other. The alkaloid was dissolved in a mixture of water, glycerin, and alcohol, having the density of distilled water, in such proportion that each fifteen minims contained one-seventh of a grain. The dose ordinarily employed was one-three-hundredth of a grain.

For Dysentery.—

℞.—Salol. . . . . gr. xxiv.
Sodii bicarbonat. . . . . ʒss.
Essentiæ canellæ . . . . gtt. v.
Mucilaginis acaciæ . . . ʒjss.
Ol. ricini . . . . . ʒiv.
Aquæ chloroformi . . . . ʒvj.—M.

S.—Two teaspoonfuls every three hours.

Rif. Med.; *Münch. med. Wochenschr.*, No. 33.

For Alopecia.—

℞.—Ol. cinnamomi . . . . ♏xx.
Ætheris . . . . . fʒj.—M.

S.—Paint once daily. The hair should be cut short and washing the head should be abstained from. From three to four weeks on an average are required to accomplish good results.

Centralbl. f. d. ges. Ther., No. 7; *Corr.-bl. f. Schw. Aerzte*, No. 16.

For Acute Articular Rheumatism.—

℞.—Acid. salicylic. . . . . ʒjss.
Alcohol. absolut. . . . fʒj.
Olei ricini . . . . . fʒij.—M.

S.—Apply topically morning and evening by means of saturated compresses, covered by an impermeable dressing and held in place by a flannel bandage.

RUEL, *Rev. Méd. de la Suisse Rom.*, 1893, No. 8, p. 484.

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ABOUT CORONERS.

THE office of Coroner is of great antiquity, and exhibits in common with other judicial offices the complex and unnecessary forms of procedure that characterized the jurisprudence of the middle ages. No doubt, in days when pathology was non-existent, violence common, and the practice of medicine carried on without organization or ethical control, it was imperatively necessary that some constituted authority, possessing compulsory process for witnesses and broad jurisdiction, should pass judgment on any cases of suspicious or obscure death. In the present state of medical science, however, the office is an anachronism, and in the corrupt politics of our larger cities is in constant danger of either abuse or neglect. It is true that since the revenues, regular or extortionate, are not usually large, and the functions of the office concern directly but a limited portion of the community, there is rarely any public discussion on the matter, but the opportunity for reform exists none the less.

At the present time considerable attention is being attracted to New York City, where, largely through efforts of the *New York Herald*, an extended exposure of Coroners' methods is being made. The agitation has resulted in a formal presentment by the Grand Jury, charging the Coroners with dereliction of duty, and advising that at the formal revision of the State Constitution, to be made shortly, the office of Coroner be abolished.

Some years ago an analogous newspaper exposure was made in Philadelphia, though the maladministration related almost entirely to questions of finance, and did not lead to suggestions of radical reform. Some years subsequently THE MEDICAL NEWS had occasion to severely animadvert upon methods prevailing in the same office in regard to the distribution of unclaimed dead, but this was remedied by the passage of the Anatomical Act.

The abuses in the New York offices (for the city has several Coroners) have been of long standing, and involve not only the despatch of business, instances of flagrant delay having occurred, but irregularities in the summoning of witnesses and in making returns to the police authorities. In Philadelphia the office has of late years been administered satisfactorily, although occasionally individual members of the medical profession have been not very kindly treated. The truth must be confessed, however, that more errors have been committed in Philadelphia by medical than by lay incumbents.

The office of Coroner is one of much greater local power than is generally supposed. It has not much patronage or social weight, but the transportation companies are usually careful to maintain friendly relations with it. A Coroner has much control over his own juries; the character of a verdict in a case of railroad accident may be of much importance in subsequent legal proceedings. Then, investigations into suspicious deaths may be pushed with vigor or be allowed to fail.

Agitation which results only in securing more efficient and honest management of the office fails to do its best. The whole system should be remodelled. The finding of a Coroner's jury is an entirely unnecessary preliminary. It is not regarded in the subsequent judicial proceedings, and many grave criminal cases are tried without any such verdict. By far the larger proportion of inquests passed upon are matters of simple routine, the cause of death being patent, while in the obscure cases the jury and even the Coroner are compelled to rely upon the testimony of experts. These experts would be equally competent to pass judgment in the simpler cases.

The Massachusetts law exemplifies the direction that reform should take. The antiquated methods of inquest and verdict are done away with; com-

petent local officials, termed medical examiners, who must be graduates in medicine, view the bodies, and, in the event of inability to determine the cause of death or of suspicion as to criminal acts, refer the inquiry to experts or to the prosecuting authorities.

The organization of local and State boards of health and the appointment of health-officers, with powers to supervise interment and to require competent certificates of death, are also conditions which render the Coroner's inquest unnecessary. It matters but little as to the title given the official who is charged with the duty of ascertaining the cause of any sudden or violent death. In Massachusetts he is called a medical examiner; he might be called a registrar or a coroner; the important point is to shake off from our procedure the absurdity of the inquest by a jury made up often of the political idlers who may be about the office and representing no social, medical, or legal constituency, dependent for their appointment on the head of the office, and in most cases forming their judgment solely on the suggestion of the experts. Questions of responsibility for murder by deliberation or neglect can be fully settled by the public prosecutor.

One point in which there is great need of increased efficiency in Coroners' inquiries is in reference to death from poison. There is little doubt that both criminal and accidental poisoning is more common than is apprehended by the public at large. Not only are many cases of this kind so obscure or so simulative of disease that no suspicion is aroused, but the methods of procedure often involve such expense that the officials are unable or unwilling to undertake investigation. Expert witnesses receive so little consideration from the courts, are put to such unnecessary inconvenience by delays in trials, that it becomes necessary for them to ask fees sufficiently large to protect themselves from absolute loss. County authorities are often unwilling to pay such fees unless it is very apparent that a successful prosecution will justify the outlay. It would be a great advantage if some systematic method of inquiry could be devised by which more frequent preliminary examinations could be made. In the sparsely settled counties it is often very difficult to get the county commissioners to authorize any unusual expenditure for analytic work, and it sometimes happens that to secure such investigations private parties, relatives of the deceased, are obliged to guarantee payment.

It is to be hoped that some day a collegiate course leading to a thorough knowledge of sanitary science and State medicine, not as a mere accessory of medicine, but as an independent degree, will be inaugurated in one of our large cities, and that the valuable material for practical instruction which the Coroner's office in such a city furnishes will be utilized in the teaching.

HIGHER MEDICAL EDUCATION.

PROFESSOR DA COSTA'S timely, earnest, and eloquent address before the Harvard Medical Alumni Association, published in THE MEDICAL NEWS of September 23d, challenges comment on several important points. We shall speak of but three of these: 1st. Preliminary training; 2d. Examinations; 3d. The university idea.

As to preliminary training, DR. DA COSTA is at one with PROVOST PEPPER and PRESIDENT ELIOT in advocating a shortening of the collegiate course in arts, if it is desired that medical students should have the training and the degree of such a course.

The University of Pennsylvania has made such a concession; and we trust that all other American universities will soon do the same.

DR. DA COSTA recognizes, however, that under existing conditions in America many men who have never entered college may be properly qualified to study medicine, and, while approving of the four years' course, he insists upon the importance of reforming medical education, not so much in the direction of lengthening the course as in securing a proper foundation, whether in or out of a college or academy. This is the position THE NEWS has steadily maintained. A man properly qualified in biology, in chemistry, and in physics can learn more in three years at a good medical school than the average student of the day, even at Harvard, can learn in five years. Begin at the beginning, then the rest will come. It is not in practical skill in medicine or surgery that American physicians are deficient, but in exact scientific knowledge. It is for this reason that, despite our LEIDYS and our DRAPERS, we have proportionately fewer great names in the sciences than in the arts of medicine, and that most of our ambitious graduates are compelled to go abroad for technical training. Increasing the number of medical students with degrees in arts will not of itself remedy this defect. We need scientific preparation. The kindergarten chemistry now taught to senior stu-

dents in many medical colleges is far less than the student ought to know before he is admitted. There is no preliminary training in biology now demanded —nor is general biology taught, except incidentally and accidentally. Yet how can human anatomy or human physiology be understood without it? As to physics, let anyone attempt to get an explanation of such simple matters as the siphon or the cupping-glass from some of the men and women to whom learned discourses on blood-pressure are to be delivered, and he will hear theories that would have amazed, rather than delighted, Pascal or Toricelli.

Dr. Da Costa does not approve of multitudinous and incessant examinations. "Why must you always pull up the student by the roots to see whether he is growing?" "We do not want to turn out medical prize oxen, stuffed and fattened for examination, but medical men, with thought, with power, with intelligence, with love of work, and of orginal investigation." There is nothing to be added to this. But it takes good teaching to turn out thinking men, while any dunderhead, with the aid of a quiz-compend, can stuff medical oxen, geese, or dummies for examination. We are afraid the examination-system will continue until we get gardeners who have sufficient confidence in themselves and in their methods to let their plants grow undisturbed.

In his remarks upon the useless multiplicity of colleges, the Harvard orator went to the root of the whole matter. So long as medical education is a business, carried on by private individuals for personal gain, whether gain in money, gain in notoriety, or gain in consultation-practice, so long will there be an insufficient preliminary standard, with its inevitable sequelæ of kindergarten lectures, cram-books, and examinations. There should be ample endowment for every medical chair, with its necessary adjuncts of laboratories and hospital beds, and the work should be done not by one lecturer, but by a large staff of able teachers with comparatively small classes. There should be no insane jealousies and envious repression of rising men, but everybody with the ability and the desire to teach should be given the opportunity, and from among those who do the best work should the promotions be made. This, as a rule, means no medical school apart from a great university, because, generally speaking, only such a university can obtain the necessary endowment; and because, moreover, several adequate and rival endowments of indispensable laboratories cannot be obtained in the same city, hardly in the same State. The time is not yet ripe, however, for so radical a measure. A gradual approach to it can be made by consolidation, as has been done in Chicago and New York within recent years.

Where consolidation is impracticable—where for instance a great school not connected with a university has glorious memories and traditions, as, for example, the Jefferson Medical College of Philadelphia—coöperation is not impossible, on some such plan as was suggested by Provost Pepper at a dinner of the Jefferson alumni. Let medical students who come to Philadelphia, said Dr. Pepper in effect, attend lectures where they will—at the University, at the Jefferson, at the Medico-Chirurgical College, or the Woman's Medical College. Let all the laboratory and hospital facilities be open to them. Let them be students of the medical faculties of Philadelphia, and let them, by having choice of teachers, stimulate the teachers to generous rivalry. Something like this seems to be the thought, too, of Dr. Da Costa, when he says: "The future of medicine lies in the universities and in such great schools as can vie with them in clinical and other facilities."

If the undergraduate and post-graduate schools of cities like Philadelphia choose to overlook mere personal interests, they have it in their power to give, by their coöperation, the greatest impulse of the century to Higher Medical Education in America.

EDITORIAL COMMENTS.

Powers of Boards of Health to Deal with Nuisances.—Several important questions have been decided by the General Term of the Supreme Court of New York in the case of the Board of Health of the City of Yonkers *v.* Copcutt (24 N. Y. Supp. 625). The testimony in this action disclosed the existence of an intolerable nuisance in the heart of that city of 30,000 inhabitants. Environed by dwellings in close proximity to its shore, there has been for many years maintained a mill-pond whose bed is covered to the depth of seven feet with foul sediment, impregnated with offscourings and putrid substances, covered at times with polluted water, and at times bare, giving off noxious, offensive, and sickening odors, which endangered the lives and health of the people. It also showed that the only effective way to abate the nuisance was to destroy the dam and enjoin the rebuilding of any obstruction to the running current of the water. The remedy of the Board of Health was, therefore, said the Court, to abate and remove the nuisance, if one existed, and charge the expense upon the owner of the land—not to order him to do it and impose a penalty for failure

to do so. However, under a statute authorizing the Board of Health to establish such regulations as might be necessary to carry into effect the powers of said Board, and to enforce observance of its sanitary code by imposing penalties, the Board could maintain an action for a penalty and injunction against the person who maintained the dam, it being declared by one section of the Board's code that anything dangerous to public health was a nuisance, and that one who supported it was guilty of violating the section, and a penalty being prescribed by another section for any violation of the code or any ordinance or resolution of the Board. But when the Board, without notice to the person to be affected, passed a resolution requiring the removal of the dam, the penalty for disobedience of the resolution could not be enforced. The fact that others beside the owner contributed to it, and even the city of Yonkers itself, by discharging sewer matter into it, did not relieve the owner of the dam from responsibility for its condition, for it was his act in maintaining the dam that kept the foul substances confined in the pond. It was also objected on the part of the owner that the Board had no power to prohibit the rebuilding of the dam and thereby prevent a nuisance, but that the Board must wait until it was rebuilt and then adjudge it a nuisance. It might as well be said that the Board could not prohibit a load of cholera-infected clothing from coming into the city, but must wait its arrival before taking measures to abate the nuisance. It was simply a means taken to prevent the increase or renewal of what had already been adjudged a nuisance dangerous to life and health. Besides, the penalty could be collected by it for violation of the resolution of the Board prohibiting the rebuilding of the dam.

Abstracting from the French as She are Done.—To make a useful and trustworthy abstract of a medical article requires a high degree of intelligence, both literary and medical, and also a great conscientiousness. One who is conversant with the originals and who is experienced in such work is frequently disgusted or amused by the slipshod quality of a large part of the "abstracting" served up by medical journals for the delight and profit of their readers. As an illustrative instance there recently appeared in a monthly medical magazine of great dignity, and with claims to *international* recognition, an abstract of an article on "Syphilis of the Liver," epitomized from the *Revue mensuelle des Maladies de l'Enfance* for January, 1893. The final sentence of the original reads:

"Dans la suite, le foie diminua considérablement de volume, au point que deux mois plus tard, il fallait le chercher sous les fausses côtes. Mais en Novembre, 1891, apparurent des gommes frontales qui vinrent prouver la nature syphilitique de la lésion."

The translator thus renders the last sentence: "About four months after operation syphilis was suggested *by the appearance of the gums!*" We are sincerely grateful for the medical abstractor's ignorance that the French word *gommes* also means *gummata*. Why did he not say *frontal gums?* Where, also, was Mr. Editor, and what was he about?

Yellow Discoloration of Hands and Feet in Enteric Fever.—Filipovich, of Odessa, notes as diagnostic of enteric fever a peculiar yellow or orange discoloration of the prominent portions of the palmar and plantar surfaces which in health usually display a pink tint. He attributes this discoloration to the enfeebled action of the heart, the incomplete filling of the capillaries, and the dryness of the skin. Filipovich found the sign constant and marked. In Philadelphia this sign has long been known to many physicians, whose attention was called to it by the late Dr. Ellwood Wilson. A valued collaborator informs us that at the Philadelphia Hospital, some two years ago, he examined the hands and feet of every fever patient in the house during a number of weeks, and found the sign present in every case of enteric fever, and, with the exception of one case of acute tuberculosis and one of acute pneumonia, absent from other cases of febrile diseases. In neither of these exceptional instances was the discoloration so marked as in the enteric cases. It is not, however, invariably well marked, even in enteric fever. The actual state of the case seems to be that the sign, while not pathognomonic, is interesting, and should count with others in making a diagnosis.

Declarations of Wounded Persons.—The declarations of a person who has been wounded, respecting the circumstances under which the wound was inflicted, are admissible in prosecutions for the killing of such person, if made at a time when he did not expect to survive the injury and all hope of recovery had been supplanted by the conviction that he would certainly die. The time when made need not be when the declarant apprehended immediate dissolution; but the declarations are admissible if made at any time when he believed that death was impending and certain. So holds the Supreme Court of Arkansas in the case of Evans *v.* State (22 S. W. Rep. 1026). It is within the province of the court to hear the circumstances under which the declarations were made and to determine whether or not they are admissible; but after they are admitted it is within the province of the jury to weigh them and the circumstances under which they were made, and give to them only such credit, upon the whole evidence, as they may think they deserve.

The Abuse of Medical Charities.—The British Medical Association has inaugurated a determined fight against the abuse of medical charities in Great Britain, and as a means to that end its committee recommends (1) that a wage-limit be established above which income persons applying for treatment will be required to pay fees; and (2) that the hospitals and dispensaries hereafter have displayed in a conspicuous place within their precincts a placard bearing the following inscription: "This institution being established to relieve only those persons who are too poor to pay for suitable medical aid, notice is hereby given that on and after this date patients will be required to give information as to their means and circumstances, with a view to prevent the abuse of this charity by persons who are well able to pay." Cannot American hospital authorities be moved to "go and do likewise?"

Admissible Expert Evidence as to Death and Nature of Blood-spots.—It is not error, holds the Supreme Court of Pennsylvania, in the case of Commonwealth *v.* Cross-

mire (27 At. Rep. 41), to permit a physician, after describing on a murder trial the injuries found on the body of the deceased, to state what, in his opinion, caused death, and how the injuries upon the person were inflicted. Nor is it error to admit in evidence his testimony to the effect that certain spots on the clothing of the person accused of the murder are blood-spots.

Honor to American Medical Investigation.—We are glad to learn that the excellent and valuable article of Professor Vaughan on "The Principles of Immunity and Cure in the Infectious Diseases," concluded in this number of THE MEDICAL NEWS, has, in advance of its complete publication, attracted some of the recognition it so well deserves, both in our own country and abroad. Dr. S. Pozzi, of Paris, has obtained the privilege of translating the address into French. We congratulate Dr. Vaughan and American medicine.

SELECTION.

STANLEY'S TRIBUTE TO SURGEON-MAJOR PARKE.

"HIMSELF depleted in body from excessive perspiration after a march through hot, moist jungles, starving on one green banana a day, half-naked from loss of clothing and the constant wear and tear of bush-life, half fainting from the pernicious ague, which could only be suppressed by heroic doses of quinin, and yet presenting to each and all of us a face whereon one read naught but pleasantness and benevolence. Pursuing steadily, day by day, throughout a period which has no equal in my life for downright utter and extreme misery, an even temper so admirable and beautiful for its sweetness and devotion, that I can think no human being, male or female, could excel it. It is here, in the possession of these natural gifts, this charming benignity, that I think Parke is great. Oh! at such a time had the ordinary human creature stood up and freed the pent-up feelings with a shout of blasphemy and explosive fury, we could not have remembered it as a sin against poor, weak, overpressed human nature; but never within my hearing or knowledge did I hear a murmur or a mean expression. That singular sweetness and tender anxiousness, having been noted by me so often when he sat in the midst of his patients unsuspecting, remain stamped on my mental image of Parke, and calling him now up to my mind I can see nothing else but that. The 'Doctori,' as the Zanzibaris called him, was to them a friend to whom they might expose their constant and ever-recurring ailments, for no one but one who was sincerely friendly could look as he did, or speak so soothingly and comfortingly.

"After his short rest he would be seen striding across the camp, always erect, vigorous, and soldierly, going from hut to hut, apparently thinking of nothing in the world but the condition of a patient he had just seen, and presently he would be bending down over another bad case with a new face and eyes which glowed with glad promise of recovery to him. The chronometer was not so faithful to Greenwich time as our doctor to his duties. No person reminded him of them; the warm interest he took in his noble profession and his own faithful, humble, and upright heart were quite sufficient. The fate of the expedition—the prospects of getting food the next day, the whereabouts of Emin or anybody else—did not appear to have that importance for him as to how Juma or Ali or Sungoro was doing. The thoughts of impending starvation and calamitous forebodings had to yield to those which included his own immediate responsibilities."—HENRY M. STANLEY, in the *Lancet*, September 23, 1893.

REVIEWS.

A DICTIONARY OF MEDICAL SCIENCE: CONTAINING A FULL EXPLANATION OF THE VARIOUS SUBJECTS AND TERMS OF ANATOMY, PHYSIOLOGY, MEDICAL CHEMISTRY, PHARMACY, PHARMACOLOGY, THERAPEUTICS, MEDICINE, HYGIENE, DIETETICS, PATHOLOGY, BACTERIOLOGY, SURGERY, OPHTHALMOLOGY, OTOLOGY, LARYNGOLOGY, DERMATOLOGY, GYNECOLOGY, OBSTETRICS, PEDIATRICS, MEDICAL JURISPRUDENCE, DENTISTRY, ETC. By ROBLEY DUNGLISON, M.D., LL.D. Twenty-first edition, thoroughly revised and greatly enlarged, with the Pronunciation, Accentuation, and Derivation of the terms, by RICHARD J. DUNGLISON, A.M., M.D. Philadelphia: Lea Brothers & Co., 1893.

IF hereafter physicians err in matters pertaining to medical lexicography it will certainly be their own fault. From the press of the present and of late years has been poured a steady stream of excellent books, from the splendid works of Foster and the Sydenham Society to those of Billings, Keating, Thomas, and others. Throughout all these years the fine old volume of Dunglison has kept its place, despite needed revision long delayed, as a classic and reliable helper of the practical and literary physician in his struggles with the thorny subject of medical nomenclature. At last the scholarly son of the great lexicographer has felt the challenges of many rivals and has put his own hand to a revision of the father's handiwork, seeking to bring to it the almost riotous wealth in the fields of new word-coinages and medical progress that has accumulated in the years since the last edition.

Few know or can even faintly realize the labor that such a resolution necessitates upon the part of the conscientious man. The verification of the mere form of a word, a date, an etymology, or a definition may mean an hour or hours of search, letters to half a dozen scientists, and perhaps at last doubt and disappointment. The delver in these dark fields surely deserves a far greater reward, both in gratitude and in hard cash, than he will ever receive. He must be content to go on his toilsome way, cheered only by the thought that conscientious work somehow brings its own reward, and that thousands of minds will be a little clearer and better as a result, direct or indirect, of his years of patient and fatiguing work. For there is a deep and widespread educational value attached to this dictionary work that few recognize. The medical profession owes Robley Dunglison a great debt for clarifying and definitizing the minds and concepts of many thousands of its younger men, for sparing young and old much intellectual vagueness, much misuse of its greatest profes-

sional tool or instrument, and much loss of labor and of time. In the face of such labor, criticism must be motived by the best kindness; it is impossible to carry out such a huge undertaking without numerous omissions and the creeping in of errors, and all reviews should be written with the idea of leading to the correction and improvement of future editions, or of warning the public against mistakes so long as they must remain uncorrected. Moreover, in the mind of the critic there must rise the thought of the interminable and exasperating differences of opinion among the best philologists pertaining to almost every word that one has to decide about.

Although thus minded, we must nevertheless remember the sharp limits of our space which prevent even mention of the many and most excellent things that might and should be enumerated: These must "go without saying," and only a few minor points noted wherein we differ from Dr. Dunglison.

The most astonishing mistake made is not printing Greek etymologic terms in the Greek characters. Epsilon can thus not be distinguished from Eta, nor Omega from Omikron. Every consideration, scholarly, literary, or scientific rebels against the plan pursued. But when decided upon, a definite rule should have been followed; the Greek letter Upsilon, for example, should have been uniformly represented either by the English *u* or by *y*, and not haphazard by either, as, *e. g.*, *pharynx* or *pharunx*. Moreover, the plan makes it impossible to say from what language the word is derived, whether from the Latin or the Greek. Then it lands our author in another difficulty, illustrated for example by the derivation of words meaning pus by four different somewhats on the same page: "*pyo*," "*pyon*," "*puon*," and "*puros*."

In pronunciation, of course, the rule, *de gustibus*, holds, but here, too, we should have adherence to *some* rule. We cannot understand why *psych-* is first *sik-*, then *si-*; *hyper-*, first *hi-per-* and then *hip-er-*; *micro-*, first *mik-ro-* and then *mi-kro-*; *gyne-*, first *gi-* and then *gin-* (both, too, wrong, as the proper pronunciation is *jin-*), *phyto-*, first *fi-to-*, then *fit-o-*, *bio*, first *be-o-*, then *bi-o-*, *dia*, first *de-ah-*, then *di-ah-*, etc., alternately and haphazard until one is sadly confused. We sincerely regret to say that the pronunciation throughout is unsatisfactory. It will be long before Dr. Dunglison gets either good or bad speakers to pronounce as he advises, *e. g.*, *pe-a'-me-ah* (pyemia) (why not *sep-te-sa'-me-ah* also?), *kar-e-o-kin-a'-sis* (karyokinesis) *pe'-ah mat'-er* (pia-mater—also strangely translated, "delicate mother"), *em-is-sah'-re-um* (emissarium), etc. A large number of words are not pronounced at all, and the one possible wrong pronunciation is too often chosen. Thus, there are several pages of *hæmas* and *hæmos* all pronounced *he-*. Suddenly we come upon our old friend (or enemy) *hæmorrhage*, and the pronunciation is as suddenly omitted; we are strangely referred to *hemorrhage*, where we get *hem*, and haul up with AHEM! There is a similar history with *hæmorrhoid*, (*he-*), *hæmorrhoids*, (*hem*), *hemorrhoids*, etc. We do not understand the significance of *h* in the phonetic pronunciation of all words with the Greek aspirate, *þ*—as, *e. g.*, in all the *-rhages* and *-rheas*.

We think the editor, if adhering to the old spelling, should have been consistent. If *fæces*, why then *defecation*? If all *hæma-*, *hæmo-*, etc., why an exception of *hemorrhage* and *hemorrhoids*? Why, oh! why does the author on the title-page spell *gynecology* and *pediatrics* as here written and stultify himself in the body of the book by spelling these words in the medieval way. It is trifling and absurd to thus "blow hot and cold" at the same time and in the same book. We also think the new spelling of words in chemistry, etc., recommended by so many learned societies, used by so many scientific writers, adopted by so many great lexicons, should at least have been mentioned.

We have noticed a number of minor slips, to which we will not here refer. But we must protest against the identification of *albumen* and *albumin*; even the lay Webster does not make this error. A number of eponymic diseases, such as Darier's, Morand's, Quincke's, Rivolta's, etc., are not to be found; a great many *images* are omitted; erythromelalgia is not a synonym of "*Reynaud's*" disease, nor of *Raynaud's*; Reichmann's disease is not necessarily followed by ulcer; several elements recently discovered are omitted from the table; several knots, all tests of birth and of death; several anesthetics and anesthetic methods (inhalers, Hyderabad, etc.); all of Weissmann's terms as to heredity (id, idant, biophore, determinants, etc.), and a number of hernias are absent. The table of bacteria is very incomplete. Hermaphroditism and an hermaphrodite are not identical. We are also sorry not to find such words as gastrodiaphany and gastrodiaphanoscope, obligate (applied to parasites), control-animal (or experiment), Laveran, cross-circulation experiment, etc.

Such things as these, however, may be expected in the best of books, but in spite of them the learned editor is to be congratulated upon the excellence of his book and upon the satisfactory completion of his arduous labors.

A HANDBOOK OF LOCAL THERAPEUTICS. By ALLEN, HARTE, HARLAN, AND VAN HARLINGEN. Edited by HARRISON ALLEN, M.D. 8vo, pp. 500. Philadelphia: P. Blakiston, Son & Co., 1893.

THIS book, under the efficient editorship of Professor Harrison Allen, is amply fitted to fill the place in therapeutics for which it was prepared. In looking over its pages one cannot fail to be impressed with the great number of drugs that have found a place as remedial agents in local affections, and the grouping together of such agents must necessarily be of value to the specialist in medicine in the saving of time and in a concentration of knowledge of the therapeutic measures at his disposal. The surgeon, the dermatologist, the otologist, the laryngologist, the oculist, and the gynecologist can each here revel in a mine of information without being compelled to wade through a mass of absolutely irrelevant material. An index of remedies is supplemented by one of diseases, both of which conduce to ready reference. A few relatively unimportant errors and omissions have necessarily crept in. Thus, in treating of the management of acute gonorrheal vaginitis no mention is made of the truly valuable results that have followed the application to the entire vagina of strong solutions of silver nitrate, and while bran as a surgical dressing has received notice, the value of the

bran-bath as an emollient in ulcerated and irritable conditions of the skin is not set down. The editor and his collaborators have completed their work in an excellent manner.

THE DISEASES OF THE NERVOUS SYSTEM: A TEXT-BOOK FOR PHYSICIANS AND STUDENTS. By DR. LUDWIG HIRT, Professor at the University of Breslau. Translated, with permission of the author, by AUGUST HOCH, M.D., assisted by FRANK R. SMITH, A.M. (Cantab.), M.D., Assistant Physician in the Johns Hopkins Hospital. With an Introduction by WILLIAM OSLER, M.D., F.R.C.P., Professor of Medicine in the Johns Hopkins University, etc. With 178 illustrations. 8vo, pp. xv, 683. New York: D. Appleton & Co., 1893.

IN less than two years several neurologic text-books have been presented to the profession by American publishers; but we cannot have too many, if they are good, and each presents some special feature. Hirt's treatise, known to the German profession since a few years, is now presented to English-reading physicians in an admirable translation. It is a work of great merit and contains some strikingly original features, as in its manner of classification. Tabes dorsalis and dementia paralytica are placed under diseases of the general nervous system, instead of under spinal affections and mental diseases, a natural and philosophic method of classifying and one that could be extended much further than it has been by Hirt. In the discussion of special subjects, and in remarks on treatment, the book in many places presents a freshness and vigor that are often wanting in compendious text-books. Certain subjects, as myxedema, have been omitted, although akromegaly and other recently described affections are considered.

Part II, which is concerned with diseases of the cranial nerves, is especially full. Under diseases of the vagus, Hirt treats of numerous nervous affections of the air-passages, angina pectoris, nervous palpitation of the heart, tachycardia, cardialgia, nervous dyspepsia, esophagismus, exophthalmic goiter—a somewhat unusual but scientific collocation.

Particular attention is given to therapeutics. The illustrations are excellent and many of them are new. The book is produced in admirable style.

THE THROAT AND NOSE AND THEIR DISEASES. With one hundred and twenty Illustrations in Color, and two hundred and thirty-five Engravings, designed and executed by the Author, LENNOX BROWNE, F.R.C.S.E., Senior Surgeon to the Central London Throat and Ear Hospital, etc. Fourth edition. Philadelphia: Lea Brothers & Co. London: Baillière, Tindall & Cox, 1893.

THE special features of this work have been described and commented upon in noticing previous editions. This edition has been revised and brought up to date; but, possibly, in order to save alterations of stereotype plates, the additions are in some instances awkwardly joined. We notice in passing, a misquotation on p. 514, whereby the substitution of "and" for "or," in the citation of Cohen's opinion on the subject of laryngeal neuroses, entirely changes the meaning of the authority quoted, who certainly did not say that *he* could not recognize any difference between the cadaveric and the extremely abducted position of the vocal bands. As a matter of fact the difference is easily recognized and is important in diagnosis. In the copy of Mr. Browne's book before us the tinting of the colored plates is not so good as in previous editions. The pinks are too brownish.

Judging the book as a whole, it is an admirable presentation of its subject in the light of the large clinical experience of a careful observer. It is a book that no specialist can afford not to have, and that the general physician can rely upon as a safe guide and practical adviser.

CORRESPONDENCE.

HAIG'S URIC-ACID THEORY.

To the Editor of THE MEDICAL NEWS,

SIR: In the article entitled "Haig's Uric-Acid Theory" in THE MEDICAL NEWS of September 16, 1893, p. 310, appears the following: "Already his work has had considerable influence among physicians, and everyone who reads his fascinating pages must feel a strong inclination to acccpt his sweeping generalizations, which ascribe to uric acid human disorders of every complexion." I shall endeavor to show that an excess of uric acid in the blood is the cause of many and diverse human disorders. If Alexander Haig had done nothing more than to direct the attention of the medical world to the dire effect of an excess of uric acid in the blood, great would have been his work.

Just one hundred years ago, in 1793, Forbes Murray affirmed the starting point of gouty symptoms to be the presence of uric acid in the humors of the economy. In 1874 Murchison announced that the excess of uric acid was due to functional derangement of the liver. In these later days Haig writes, elucidating the action of uric acid in the causation of disease.

What disease is more common among adults in America than lithemia, presenting itself under so many various disguises. If it be true that Americans are a nation of "nervous prostrates" (and so it appears), then common indeed is lithemia. Take for example insomnia, a condition far from rare in city life, so frequently *made worse by hypnotics*, so quickly relieved when once the true cause is recognized—a sleeplessness due to derangement of the liver, producing lithemia. In such a case sodium salicylate in combination with caffein citrate yields excellent results.

Then, again, megrim, so very common, is quickly relieved by the same agents as were so useful in insomnia, when the cause is rightly understood as simply a lithemic crisis. The neurologist, Sachs, says that the same form of treatment that had been successful in a number of cases of insomnia proved successful in most of the cases of megrim with which he had to cope.

In acute tonsillitis, always found in subjects whose urine contains an excess of uric acid, I know of no treatment offering better results than sodium salicylate. The same treatment is likewise beneficial in both acute gout and acute articular rheumatism, both of which are due to the same cause—an excess of uric acid in the blood.

At the first annual meeting of the Association of American Physicians, held in 1886, Dr. H. C. Wood (THE MEDICAL NEWS, June 26, 1886, p. 716) said that it was his experience that rheumatism and gout are the same disease.

At the same meeting Dr. Draper, an authority on gout in America, said he thought Dr. Wood was correct in regard to the difficulty of diagnosis between gout and rheumatism. He would be glad to hear from any physician present who was always able to diagnosticate between the two.

Neuralgic pains, so annoying when due to an excess of uric acid; muscular rheumatism, a manifestation of lithemia; gravel, and the painful urethritis so often an accompaniment; and general pruritus, so often due to an excess of uric acid, are all relieved by the administration of sodium salicylate.

Oftentimes palpitation and irregularity of rhythm of the heart are produced by the state of the blood (Garrod); also the minor symptoms of disturbed action of the heart, such as giddiness and dimness of vision (Duckworth); the most prominent symptoms are the nervous, especially those referred to the head, in those cases in which uric acid is present in excess in the blood.

The mal-products of digestion are positive depressant poisons (Brunton); hence lithemic patients present themselves as woful objects; they are in dread of apoplexy or are sure that they are developing paresis, or they are insufferable cranks.

The functions of the liver and the kidneys are very closely related (Delafield); so that what starts as a mere functional disorder of the liver will in time, if not checked, end in organic disease of the kidneys. "Renal degeneration is a consequence of the long-continued elimination of products of faulty digestion through the kidneys"; so that what originates as lithemia often terminates as uremia. It is well to remember that uric acid is a "kidney irritant"; also that uric acid "teases the whole urinary tract from the tubules of the kidney to the meatus"; also that an excess of uric acid is a frequent cause of various ailments. The whole medical world is greatly indebted to Murray, Murchison, and Haig.

Respectfully,

H. A. SHELLEY.

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NEWS ITEMS.

Dr. D. W. Montgomery has been elected Professor of Diseases of the Skin in the University of California.

W. Moldenhauer has been made Professor Extraordinary of Laryngology and Rhinology at the University of Leipzig.

A Municipal Hygienic Institute has been established at Hamburg and placed under the charge of Dr. Dunbar, a former assistant of Prof. Gaffky, at Giessen.

Charles Clay, known as the father of ovariotomy, died recently at Poulton-le-Fylde, near Blackpool, England, at the ripe age of ninety-two years. He performed his first ovariotomy in 1842, with success. In 1845 he extirpated a uterine fibroid through an abdominal incision. He was a prolific contributor to medical literature.

Dr. William B. Towles, Professor of Anatomy and Materia Medica in the University of Virginia, died September 15, 1893, at the age of forty-six years.

Cholera at New York Harbor.—Bacteriologic examination has disclosed the presence of comma bacilli in the dejections of a case of fatal illness on board the steamer Russia from Hamburg, detained in quarantine at New York Harbor. There were several deaths during the journey, but the disease was not believed to be cholera.

The American Electro-therapeutic Association at its third annual meeting, recently held at Chicago, elected the following officers: President, Dr. W. J. Herdman, of Ann Arbor; Secretary, Dr. Margaret Cleaves, of New York; Vice-Presidents, Dr. Franklin H. Martin, of Chicago, and Dr. A. Lapthorn Smith, of Montreal; Treasurer, Dr. R. J. Nunn, of Savannah, Ga. It was decided to hold the next meeting in New York City, on the last Tuesday in September, 1894.

A Week on Cataract.—The Philadelphia Polyclinic will devote a special week, beginning October 30th, to the consideration of cataract. Cataract-operations will be performed by the different methods preferred by Professors Harlan, Jackson, Risley, and de Schweinitz, including operations with iridectomy and simple extraction. The whole subject of etiology, diagnosis, and the dressing and after-treatment will be considered.

In addition to the clinics, demonstrations, and practice of operations on the eyes of lower animals, there will be a series of clinical "conferences," participated in by the professors and the members of the class.

Opening of the Johns Hopkins Medical School.—The regular courses leading to an M.D. degree at the Medical School of Johns Hopkins University opened on October 5th with a class of sixteen, of whom three are women. The high standard set by Johns Hopkins for admission to this school has, for the first time in the United States, secured a band of medical students each with a bachelor's degree from some reputable college. Of the thirteen men, Johns Hopkins supplies four, Yale three, Harvard two, and Amherst, Swarthmore, Virginia Agricultural College, and the University of Wisconsin each one. The women are graduates respectively from Smith, Vassar, and Wellesley.

BOOKS AND PAMPHLETS RECEIVED.

Diseases of the Rectum and Anus: their Pathology, Diagnosis, and Treatment. By Charles B. Kelsey, A.M., M.D. Fourth edition. New York: Wm. Wood & Co., 1893.

Transactions of the Fourteenth Annual Meeting of the American Laryngological Association, held in the City of Boston, June, 1892. New York: D. Appleton & Co., 1893.

International Clinics: A Quarterly of Clinical Lectures on Medicine, Neurology, Pediatrics, Surgery, Genito-urinary Surgery, Gynecology, Ophthalmology, Laryngology, Otology, and Dermatology. Edited by John M. Keating, M.D., LL.D.; Judson Daland, M.D.; J. Mitchell Bruce, M.D., F.R.C.P.; David W. Finlay, M.D., F.R.C.P. Vol. I. Third series. Philadelphia: J. B. Lippincott Co., 1893.